# SCIENCE

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## *SCIENCE AND LIBERAL EDUCATION*[1]

SEVERAL years ago a discussion was carried on in one of the London newspapers on that interminable but always interesting question as to what is the best definition of a gentleman. Various answers were suggested by different contributors. Some were in the form of citations from our noblest literature—one, as I recall, was given in the words of St. Paul, another was taken from Shakespeare, a third from Emerson. The one generally acknowledged to be the most effective was, however, phrased in the picturesque vernacular of modern sport. A gentleman, so this answer ran, *is a man who plays the game.*

As this lingers in the memory it brings a growing sense of broader implications. The definition, evidently, only gives a new turn to the old thought that human life is like a great game that man plays with the world. We recall the striking words in which an illustrious master of modern science once brought this thought to bear upon the problem of education:

> The life, the fortune and the happiness of every one of us depend on our knowing something of the rules of a game infinitely more complicated and difficult than chess. It is a game which has been played for untold ages, each man and woman of us being one of the two players in a game of his or her own. The chess-board is the world, the pieces are the phenomena of the universe, the rules of the game are what we call the laws of nature. The player on the other side is hidden from us. We know that his play is always fair, just and patient. But also we know, to our cost, that he never overlooks a mistake or makes the smallest allowance for ignorance. To the man who plays

[1] An academic address given at the opening of Columbia University, September 29, 1915.

well the highest stakes are paid, with that sort of overflowing generosity with which the strong delight in strength. And one who plays ill is checkmated—without haste, but without remorse. My metaphor will remind you of the famous picture in which a great painter has depicted Satan playing at chess with man for his soul. Substitute for the mocking fiend in that picture a calm, strong angel who is playing for love, as we say, and would rather lose than win, and I should accept it as an image of human life. . . .

Well, what I mean by education is learning the rules of this mighty game. In other words, education is the instruction of the intellect in the laws of nature, under which name I include not merely things and their forces but men and their ways, and the fashioning of the affections and the will into an earnest and loving desire to move in harmony with their laws.

And a little farther on it is added that a liberal education should teach us to love all beauty, whether of nature or of art, to hate all vileness, and to respect others as ourselves.

Huxley here formulates his view of education in words that breathe the very essence and spirit of scientific inquiry. The end of education, obviously, is not the mere acquisition of knowledge; it is the attainment of a point of view. And the value of science in this respect, as I think, depends mainly upon the attitude of the scientific investigator towards the study of nature. For he, too, is like a player in a great game. He is quite aware that he can never bring it to a conclusion or sound all of its depths. Nevertheless, he throws himself into it without hesitation, certain of its inexhaustible interest and of possibilities of achievement that are past all reckoning.

I will say but a passing word concerning the work of our professional and technical schools of science. More and more in the future the practical efficiency of our civilization will depend upon that work—in medicine and sanitary science, in agriculture and forestry, in the many branches of engineering—in all those practical disciplines that we speak of as the applied sciences. But civilization does not live by practical efficiency alone, neither is education merely a matter of vocational training. Something larger is here involved. What is the greatest service of science to our intellectual and spiritual life? And this, I take it, is only another way of asking: What is the value of science in general or liberal education?

There are certain obvious aspects of the question that will detain us for a moment only. Science should teach us to keep an open mind; to look facts straight in the face. It should help to deliver us from the deadly vice of thinking we know things of which we are really ignorant. It should lead us to place a higher valuation on observation and experiment than on authority and precedent. We should, of course, acquire some definite information concerning the material world; we should become aware of the fundamental order that is discoverable among natural phenomena; we should gain an intelligent view of man's place in nature. The biologist is apt, perhaps too apt, to emphasize the bearing of his work on problems of human life—psychological, social, political, ethical. No one supposes that all the intricacies of the social organism are at present within the reach of biological searchlights; far from it. Nevertheless, we are ourselves objects in nature and a product of natural processes. No man, I venture to think, can be called liberally educated who has been left indifferent to the issues that are here involved.

But these things, too, let us pass by; they are plain to demonstration. I ask attention to something that is, perhaps, less obvious but to my way of thinking is more important still. The main service of science to our intellectual life is to help preserve us from a certain disorder of the

imagination which I will permit myself to speak of as the malady of Peter Bell. I make no attempt to disguise the fact that Peter Bell and his celebrated primrose have begun to show the ravages of time. Even so, I suspect that science will not with impunity lay her desecrating hand upon Wordsworth's parable. And yet that perennial primrose by a river's brim, which through every changeful year

> A yellow primrose was to him
> And it was nothing more—

that weather-beaten botanical specimen, I say, symbolizes a kind of mental myopia with which the man of science feels himself to be as much concerned as the poet. Science has a very definite part to play in the treatment of this insidious ailment. It should adjust our vision to the larger meanings of things in the material world. And by this I mean to say *that science should develop—and it should discipline—the constructive imagination.*

This thought, I must admit, is very far from novel; to some, nevertheless, it may seem to be a rather quixotic notion. Let me then briefly set forth some of the grounds that lead me to adopt it. I might cite here Tyndall's historic address on the scientific use of the imagination—one with which every student of science should be familar. But lest I be accused of calling upon a prejudiced witness, I will turn to the dry light of legal authority, recalling words that were written long ago by Sir Frederick Pollock:

> It is an open secret to the few, but a mystery and a stumbling block to the many, that Science and Poetry are own sisters; insomuch that in those branches of scientific inquiry which are most abstract, most formal, and most remote from the grasp of the ordinary sensible imagination, a higher power of imagination akin to the creative insight of the poet is most needed and most fruitful of lasting work.

This was an eminently sound and upright judgment, yet the mystery seems not wholly to have been dispelled. Even now we not infrequently are asked: Is not science a negation of all that is poetic or imaginative? Is it not concerned with hard facts, with exact weights and measures, with sharply defined and rigid concepts, with the iron rule of "blind force" and "inexorable law" in nature? Well, this view of science is known esoterically, in the seclusion of the laboratory, as the Great Myth. We can make some use of it, to be sure, as a kind of polite fiction with which to introduce the neophyte to the methods of science. We glibly insist that the student must above all things learn to observe, measure and weigh with "irreproachable accuracy." With this demand he struggles valiantly but in vain; meanwhile, however, the fundamental truth begins to dawn upon him that nobody is able with perfect accuracy to observe, measure or weigh anything. We are always in error, more or less. Science—and here I mean the sciences of observation and experiment—has not the smallest hope of attaining to absolute precision. She is quite content to determine the probable limits of error!

The fundamental concepts of science are in no better case than her weights and measures. They have no finality. They are but a means of advancing knowledge; they move as science moves. But yesterday we were told that atoms are infinitesimal, indivisible, ultimate. To-day we are holding our breath as we watch the physicist soaring upon the wings of imagination inside the atom in pursuit of still more infinitesimal, still more indivisible, still more ultimate electrons which, like the stars of heaven, are rushing through the tremendous solitudes of intra-atomic space! Tomorrow, no doubt, the scene of these daring flights will be shifted to the nebulous and illimitable regions that lie still unex-

plored within the electron. What would the illustrious Dalton think of all this could he contemplate to-day the fate of his epoch-making theory after a century of scientific progress? We may imagine him saying to himself: "Well, Well! the concept of the atom seems to have made some progress since my time; evidently it still is moving onwards. And yet, the flourishing state of latter-day physics and chemistry gives me an impression that the atomic theory, with all its faults, has played a certain useful part in the advancement of knowledge."

Surely then, it may be said, we can make a final stand upon the laws of nature. Are not these immutable and eternal? Do they not govern the world as overruling necessities? Science and philosophy alike reply: Who knows? The laws of nature are known to us only as observed uniformities or harmonies of action. We can not affirm with certainty that any one of them—not the law of the conservation of energy, not even the law of gravitation—is fixed or universal. No particular law of nature—and here I employ the words of a great master of mathematical physics—will ever be more than approximate and probable, nor can we state it completely. But this, says some one, is a confession of the fundamental bankruptcy of science! No; our cynical friend goes too fast; his remark betrays a failure to comprehend either the aims of modern science or the nature of scientific progress. We have no notion that our plummets will so easily touch the bottom of nature; we do not hope to divine the final essence of things. And were it otherwise, were it possible for our knowledge of nature to be frozen, once for all, into a hard, mechanical and completed system, the most potent spell of science would be broken. A deep-seated instinct of our human nature here asserts itself; one that has found expression in the philosophy and poetry of every age. It is a saying of Confucius that they who know the truth are not equal to those who love it. Which one of us, were we forced to choose, would not in the end make the famous choice of Lessing:

> If God held in his right hand all truth, and in his left nothing but the ever-ardent desire for truth, even with the condition that I should err forever, and bade me choose, I would bow down to his left hand, saying, "Father, give me this; pure truth can be but for Thee alone."

Such a choice, however, is not very likely to fall to our lot; nor does the poet here suggest the actual attitude of science. Malbranche is nearer to it in that delightful remark that if he held truth captive in his hand he would open his hand and let it fly, in order that he might pursue and capture it again. Here we sense the spirit of the sportsman. With somewhat of the same spirit, beyond a doubt, does the discoverer play his small part in the mighty game of science.

And this brings us to the vital point. Not merely in a spirit of sportsmanship does science play her game; she also strives to realize an ideal, one that is very plain and simple. And this ideal is, in a single word, *progress*. Not to solve the ultimate riddles of the universe, if such there be, not to attain to absolute truth, but to advance knowledge—such is the aim of science. When once we have felt the full force of this, all is clear. Then we see that although the laws of nature can be formulated in only a tentative and provisional way, they are not for this reason less valuable to us; they are more so. Then we grasp Huxley's full meaning when he calls these laws the rules of the game we play. We can not with certainty ascribe to them any quality of necessary or inflexible truth. Our knowledge of nature is of inestimable value to us; but it is nowhere absolute or final. The profound

significance of what we call natural laws lies in the fact that they tersely sum up our experience of the world at any given moment; and, above all, they endow us with a gift of prophecy that leads us on to new advances. Just here we are in sight of what is most vital, characteristic and hopeful in the spirit of modern science; and here, too, the all-important rôle of the scientific imagination first begins really to impress us.

Our assured knowledge of nature is gained little by little, through the slow and cautious processes of observation, experiment and reason; but far different from these is the motive power of science. In every field the great discoverers have been seers, men of imaginative vision, carried onwards by a swift intuition that runs far in advance of solid fact or rigorous logic and ranges freely to and fro in undiscovered realms beyond them. And this is a true creative process, one that is singularly like what we call the inspiration of the painter or the poet. It often thrills us in the same way. Such a work of the imagination was Michael Faraday's wonderful anticipation of the electro-magnetic theory of light. Such was Charles Darwin's conception of natural selection which, as he himself has related, suddenly flashed upon his mind as he was reading the famous book of Malthus. Such, again, were the dreams that led Louis Pasteur and his followers, step by step, from the phenomena of fermentation and putrefaction to that most beneficent and practical achievement of our civilization—the germ theory of disease. At every point the material world overflows with half-revealed meanings about which science is forever weaving her imaginative fabrics; and at their best these have all the freedom, boldness and beauty of true works of art. One conspicuous trait, indeed, distinguishes the man of science—his incorrigible, almost automatic insistence upon verification. For no one better knows that the children of his imagination will live only in so far as they take on the living flesh and blood of reality in the appeal to nature. Not many of them survive the ordeal; yet they are the pioneers of progress, and the real conquerors of the world. Our own alma mater has placed above the portal of one of her great halls of science the words: *"Speak to the earth and it shall teach thee."* Beside them we might fittingly inscribe that other Scriptural admonition: *Prove all things; hold fast that which is good.*

I say once more, then, that the development and discipline of the imagination is the best gift of science to our intellectual life, and hence to liberal education. Permit me also to suggest that there is no royal road, no pedagogical short-cut, that will lead us to it. Lectures and text-books on science are, I suppose, a necessary part of the apparatus of educational nutrition. Not by them, however, but by the actual phenomena of nature is the scientific imagination first awakened to its real life. Our laboratories of science have their shortcomings; in them, nevertheless, such as they are, the fruitful and abiding lessons of science are learned. It must not be imagined that the student has no other occupation than to disport himself at ease in that legendary realm of popular fancy known as the "fairyland of science." The prosaic but wholesome truth is that most of us are kept too busy digging out facts at lower levels to have much opportunity to breathe the atmosphere of the upper altitudes. But if the learner can not be taken up straightway to the highest mountain peaks, he may at least be enabled now and then to catch glimpses of them from the quarries in the foot-hills where most of us are toiling. And such moments of larger

vision repay with overflowing measure all the labor they have cost, for it is then that the miracle is wrought and the eyes of Peter Bell are opened.

Permit me one more word. Science lays her spell upon us because she lives and moves. It ought to be clear that the advancement of knowledge is not less vital to the educational interests of the university than are its conservation and dissemination. Are we quite sure of ourselves in this regard? We have heard of late an intimation that the universities have not been so much leaders of progress as "depositories of stationary thought." Well, depositories of stationary thought the universities indubitably have been, like the monasteries that they succeeded as centers of learning; and they have thus served as the guardians of a treasure that is beyond all price. But this is only half the truth; for it has long been one of our most cherished ideals that universities should also be the natural homes of original discovery and productive scholarship. The real universities—and I believe that our own is one of them—have demonstrated by their example that the atmosphere which these things create make teaching live and move. But even as we are insisting upon this we find ourselves wondering how our ideal is likely to fare hereafter in the continual expansion of modern universities and the multiplicity of new demands upon their teaching resources. Our pedagogical and executive machinery is admirably organized. It has developed a high degree of efficiency. Will it be efficient enough in the future to maintain an atmosphere in which scientific research and creative scholarship may freely breathe? It is easier to ask hard questions than to answer them. This one, nevertheless, we shall not escape; for the day is coming when the leadership of the universities in intellectual progress will depend on the reply that we and those after us shall make; and not our words, but our deeds will speak for us.

Edmund B. Wilson
Columbia University

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## *PLAIN WRITING*[1]

Two years ago I spoke to the American Mining Congress on the subject "Plain Talk"—both preaching the use of direct statement and trying to practise what I preached. Of late my thoughts have turned more and more to the need of the use of popular language in stating technical results; hence this afternoon I venture to discuss plain writing from the standpoint of a government scientist. For twenty-odd years my association with scientists has been fairly intimate, and though I may not qualify in plain writing myself, I can claim large acquaintance with both the written and the printed page whose meaning is far from plain.

At its best, science is simple; for science is not much more than arranging facts so as to set forth the truth. Scientific thought is exact and direct, and scientific writing must therefore be accurate and to the point. The scientist should think directly and with the precision of one of the instruments of his trade, and above all his language must present that thought exactly.

In scientific writing this need of exact statement has led to the use of special terms, words that keep their razor-edge because used only for hair-splitting distinctions. In a certain degree this adoption of words not commonly used is unavoidable and therefore defensible. Yet the practise is carried to an extreme and far too often the result is a highly specialized language so distantly related to our mother tongue that as a preliminary qualification the writer has to pass a civil-service examination and the reader usually finds himself "shut out" and facing a "no admittance" sign unless he happens to possess the degree of Doctor of

[1] Meeting of American Mining Congress, San Francisco, September 13, 1915.

Philosophy in that particular branch of science.

Mind you, it would be folly to throw away these tools so well fitted for special purposes; yet it is no more the part of wisdom to put them to every-day uses. The task for the scientist is to decide when to use his technical terms and when to talk United States. Of course, any writer's first duty is to be intelligible. Choice of language thus resolves itself largely into an understanding of the audience. If a scientific investigator desires to announce his discovery to his fellow workers, he does well to use those exact terms that carry the same shade of meaning the world over and indeed may have the same form in several languages; if, on the other hand, his results have immediate value for the mine operator or the prospector, the geologist does not and can not accomplish his purpose unless he writes in plain language, using words possibly less exact but surely more understandable.

It may be that I have stated the case too simply, so that this matter of plain writing may seem altogether easy, yet making out the prescription is always much easier than effecting the cure. Indeed, I suspect the difficulty is largely an internal trouble with the author, so deep-seated that my simple remedy of fitting the language to the reader does not reach it.

Sir Clifford Allbutt in his "Notes on the Composition of Scientific Papers" lays down the plain rule: "Take pains, therefore, with yourself first, then with your reader." His idea that clear thinking must be the first step to plain writing of course deserves our endorsement, based upon experience. How common is the sad discovery that a piece of obscure writing is simply the product of roundabout reasoning or twisted thinking. Printer's ink, in whatever amount used, unfortunately possesses no magic properties as a reagent for clarifying muddy thoughts. Yet no doubt it sometimes happens that some of us try to cover up with long words our uncertainty in thinking. So in preaching reform in scientific publications, those of us who are doing the work must realize that plain thinking comes first. There's the rub!

It is therefore not a coincidence that some of the deepest thinkers in geological science have also possessed a literary style conspicuous for clarity of expression. On the other hand, some authors whose English needs the most editing are equally careless in the quotation of facts determined by others and indeed in the statement of their own observations. I mention this simply to show that I am strong in my belief that plain writing is not something beneath the plane of endeavor of the scientific investigator—indeed, it is something so hard to attain that the most of us need to aim high, to raise our standards of scientific thinking. The use of common words is worthy of any writer if his purpose is to transmit thought.

The discussion of plain writing at this time is not academic, because my real purpose is to take this opportunity to announce to you the policy of the United States Geological Survey on this subject. Our explorations, surveys and investigations are in the public interest only as results are made public. This policy is as old as the Geological Survey itself, but several things have given a special impetus to the development of this policy. Beginning in August a year ago, a large volume of inquiries from producer and consumer of minerals came pouring into our office, and as never before the Geological Survey became a kind of "central" to the mineral industry. This opportunity for a larger service to the public not only resulted in gratifying relations with a large number of correspondents, but the rendering of such service has proved instructive to the public servants charged with the duty. Many of us on the Survey staff have acquired a keener realization of the need not only of giving the public the facts, but also of making those facts intelligible and useful to the citizen who may lack professional training in geology or engineering.

Another line of this larger service has been the issue of four guidebooks to this great western country, in which the purpose has been to inform the traveler concerning the resources of this part of our country as well as to unfold to him in attractive form its fascinating geology. The effort to meet the public need of

authoritative information of this type seemingly has met with success, and other guide-books in this series will follow in other years. More than that, however, the reflex influence of this innovation is already felt, and the evident appreciation by the general public of this type of popular description is encouraging the Survey writers. The educational responsibility of this federal service is being more fully realized, and we intend to give much more attention both to the simplification of the language of the professional publications and to the issue of reports that shall be popularly descriptive and instructive without loss of exactness. Even if plain language is used our reports should be no less efficient vehicles for professional discussion or for the announcement of geologic discoveries.

For thirty-six years the United States Geological Survey has reached an ever widening circle of readers, and even in the early years of the Survey's life King and Emmons and Gilbert gave to the West the results of their work in strong and forceful English. Yet with the growth of the organization and the development of the science the tendency toward highly specialized writing has been too marked and the present plea for plain writing has become necessary. The government scientist has at least two obligations: first, that of making his investigations more and more exact in method and direct in result; second, that of making his product, the written report, such as to meet the needs of not only his professional associates but also the general public. It is our ambition that the reports of the United States Geological Survey shall be written in the language of the people.

George Otis Smith
U. S. Geological Survey

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## *SOIL FERTILITY*

As long as a soil continues to produce moderate crops, the question of its fertility arouses no concern, but when the yield falls below normal the reason for this decrease is immediately sought. Until a short time ago it was believed that this difficulty admitted of an easy solution, but when the farmer saw his crops decreasing and sought the cause, the type of answer which he received depended on whether he consulted a soil physicist, a soil chemist, or a soil bacteriologist. In any case it was generally conceded that the supply of "plant food" had been exhausted and the only question remaining was how to replenish it.

The soil physicist saw in this undesirable condition, from his standpoint, a violation of the maintenance of one of the following requirements for the soil under examination. First, the proper temperature had not been established in the soil to admit of the rapid growth of crops; second, the proper ventilation of the soil had been interfered with, either by a change in the porosity of the soil due to physical or chemical changes, or to the deposition and retention of the by-products of the crops; or third, the plant did not receive sufficient moisture and this was due to the non-operation of one of the following agencies, osmosis, surface-tension or transpiration. The importance of this third point is very apparent when we remember that all plant food taken from the soil must be in solution.

The following quotation from Johnson's "Agricultural Chemistry" illustrates the standpoint of the chemist of a few years ago in regard to soil problems.

> The art of culture is almost entirely a chemical art, since nearly all its processes are to be explained only on chemical principles. If you add lime or gypsum to your land, you introduce new chemical agents. If you irrigate your meadows, you must demand a reason from the chemist for the abundant growth of grass which follows.

The extension of such ideas as are contained in the above quotations led to the belief that there is a certain definite relation between the productiveness of the soil and its content of nitrogen, potash, phosphoric acid or other chemical constituent, and many persons believe at the present time that from a chemical analysis of a soil the analyst can tell just the kind and amount of fertilizer to be added in order to increase its productiveness.

With the introduction of more exact methods in bacteriology and the perfecting of bacteriological technique, all of which has taken place

in the last half century, the relation of bacteria to soil cultivation has been carefully studied. There are myriads of bacteria in the soil and from the standpoint of the agriculturalist, they naturally fall into two classes: Those concerned in increasing the fertility of the soil, and those which deplete the soil of the necessary constituents to sustain plant life. Two groups of bacteria (*Clostridium* and *Azotobacter*) have the ability of taking nitrogen directly from the air and incorporating it into their own protoplasm, and it is believed that these organisms are responsible to a large extent for keeping the nitrogen content of a soil nearly constant, as they replace that lost from the soil by the removal of crops, dentrification (which will be considered later) and seepage. Another method by which the nitrogen content of a soil is increased is that of a symbiotic relationship between certain bacteria and plants; *Ps. radicicola* has the ability of penetrating the roots of the legumes and forming nodules thereon. Under this special condition, the bacteria above mentioned are able to "fix" atmospheric nitrogen. This element is rarely, if at all, available to plants in the form of ammonia, but must be built into nitrates first. Here other types of bacteria (nitrifying organisms) take the "fixed" nitrogen and build it up to nitrites and nitrates.

It is upon these facts that the excellent results of crop "rotation" depend and this is responsible for the success in intensive farming. In the "rotation" of crops the farmer sows the land between "money crops" with one of the legumes (peas, beans and especially clover are used) and these plants are those above referred to as being capable of a symbiotic relationship with the nitrogen-fixing bacteria. When these legumes have grown up, they are ploughed under, thus returning much nitrogen, which has been accumulated during their growth to the soil, and replacing that lost by the removal of the "money crops." This process is known as "green manuring" and it is fast becoming the practise to spray the seeds of these legumes with pure cultures of nitrogen-fixing organisms, so that the proper bacteria may be present when the seeds are planted.

The question naturally arises that if nitrogen must be in the form of nitrates in order to be available to plant life, why is it that we often add materials as fertilizers which contain nitrogen in a much more complex form? Bacteriology again answers this query. These complex nitrogenous substances which we add as fertilizers are broken down by the saprogenic and saprophilic bacteria to the ammonia stage from which they are built up, through the agency of the nitrifying bacteria to the nitrite and finally nitrate form, and are now in a condition to be assimilated by plant life. Through the agency of other bacteria cellulose, starches and sugars are transformed to organic acids and carbonic acid, which attack otherwise insoluble minerals and get them into solution ready for absorption by the plant. Bacteria are also involved in the oxidation of hydrogen sulphide and iron compounds.

So far, we have discussed only those organisms which are beneficial to plant life, but there are others which are detrimental and these naturally fall into two classes; first, those bacteria which are pathogenic to plants, like those causing tobacco and cucumber wilt, potato rot, pear blight, etc., second, those bacteria which are indirectly harmful to plants because they rob the soil of the constituents necessary to support plant life. These latter fall in the group above referred to as denitrifying organisms and they have the ability to break down nitrates, either partially or wholly to the ammonia stage, thus robbing the plant of what was originally available nitrogen. This type of organism was much feared by the agriculturalist for some time after its discovery, but it is now known that it requires an entirely different environment from the nitrifying organisms and that it seldom occurs in a well-ventilated soil.

The single view of the soil physicist must appear untenable, for while plants need water, air and a proper temperature, these are not the only requirements to be met, for if it is desired to have a fertile soil to insure our crops, "plant food" must be present and also the

agencies whereby this food will be replaced when that which is now present is consumed. We must also recall that water, air and temperature greatly influence the bacterial content of the soil and they are probably more sensitive to changes in respect to these three requirements than are the plants themselves. As will be seen later, it is possible to meet all the demands of the physicist and yet have an unproductive soil.

The single standpoint of the chemist is also open to criticism, for, granting that the right amounts of the necessary elements are present in the soil and are in an available form at the time of examination, they will soon become exhausted unless replaced by bacterial activity. The amount and kind of material in the soil solution is one that has caused considerable discussion and for which we can set no arbitrary standards, knowing that this is intimately related to the composition of the soil, which is dependent upon the original nature of the rock of this place and the care of the land since it has been cultivated. Would it, remembering that different animals require different kinds and amounts of foods to repair their protoplasms, be quite correct to assume that all plants require exactly the same amounts of various substances to repair their equally diversified protoplasms?

While it may be claimed that the bacteriological content of a soil is a very delicate index of its fertility, we must not forget that the chemistry and physics of the soil are also important. It is true that from the type of bacteria present we can form a good idea of the fertility of the soil, but without the knowledge gained from a physical and chemical examination we have no means of knowing how long these conditions will persist.

The problem of soil fertility then is a composite one which needs for its solution a knowledge of the interrelated subjects physics, chemistry and bacteriology. With these points in view, it becomes a very simple matter to harmonize statements which on the surface seem conflicting. We now know that it is necessary to provide the proper laboratory (physical conditions) and the necessary raw material (chemicals, etc.) in order that the particular bacterial cell which we desire may do its work in increasing the fertility of the soil. In order to show the dependence of the bacterial cell on physical factors, we may cite the physicist's demand for a porous soil and remember at the same time that the organisms beneficial to plant life require oxygen in order to continue their reactions. Again the necessity for moisture is clear when we remember that bacteria can use only food that is in solution, and the question of a proper temperature is explained when we remember that at the optimum temperature of the bacteria in question, their reactions are greatly accelerated.

To discuss in detail the tenets of the chemist would require too much space here, but one simple illustration may suffice to show the close relationship between the chemical composition of a soil and its bacteriological content. The adding of chemicals to a soil affects the physical and bacteriological nature of the soil as well as the chemical content. When we remember that the activity of bacteria often produces acid end-products, which if not neutralized will inhibit their activity and finally cause their death, the reason for the addition of lime to a soil is easily understood.

If we provide all the conditions above outlined and the soil continues to be unfertile, we may be sure that we have not the proper bacteria present or else some enemy such as certain protozoa are present and are preying on those bacteria which make plant life possible upon the earth. As animals are in the last analysis dependent upon the plants for food, and as plants are dependent upon the nitrifying bacteria, we can easily see that no life could long exist upon the earth without the aid of these organisms.

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Carnegie Institute of Technology

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## *THE THIRTEENTH NEW ENGLAND INTERCOLLEGIATE GEOLOGICAL EXCURSION*

The annual meeting of the Geologists and Geographers of the New England Colleges and Universities was held under the direction of

Professor Joseph Barrell, of Yale University, on Friday and Saturday, October 15–16.

The purpose of the excursion was to determine whether or not the so-called Cretaceous peneplain of southern New England was the work of subaerial or of marine erosion, and was conducted by the author of the latter theory.

In the preliminary lecture delivered in Peabody Museum, Yale University, Professor Barrell pointed out that the importance of peneplanation in continental interiors was not questioned nor the quantitative dominance of river erosion even on the eastern side of the Appalachians. From evidence based on a broad study of the country between Vermont and Virginia it was concluded that the so-called peneplain of southern New England was originally stair-like or terraced in its character, the terraces facing the sea and bearing the marks of having been developed by recurrent marine denudation. By projecting the base of the Potomac and later formations their relations were brought out with the marine terraces of Maryland. The oldest terrace is regarded as Upper Cretaceous. The Piedmont plateau is regarded as developed by alternations of marine and subaerial denudation during the Pliocene.

The party left New Haven Saturday at 8:00 o'clock for Waterbury, Conn., where automobiles awaited. The route traversed extended through Morris, Litchfield and Goshen to Torrington. Luncheon was eaten in Litchfield.

The four terraces recognized in this region by the leader, regarded as Pliocene in age, and pointed out to the party are:

Cornwall terrace, elevation 1,680–1,720,
Goshen terrace, elevation 1,340–1,380,
Litchfield terrace, elevation 1,100–1,140,
Prospect terrace, elevation 880–920.

Of these terraces the Prospect in the locality viewed was seen to have been much dissected by subaerial erosion while the Litchfield has a remarkably smooth surface. No line of cliffs separates these two terraces, the separation being determined largely from topographic maps by the rather rapid descent of the surface near the junction of the two terraces. Rather pronounced but much dissected scarps between the Litchfield and Goshen and between the Goshen and Cornwall terraces were pointed out.

It was suggested by members of the party that the terraces might have been caused either, (1) by monoclinal warping or, (2) by a difference in the hardness of the rock. It was pointed out by one of the physiographers, however, that mere difference in hardness of rock would hardly account for the succession of dissected levels which was observed. The leader regarded monoclinal warping as inapplicable when applied to the actual details as developed across the country.

In general, Professor Barrell holds that the highest or Cornwall terrace was first cut by the marine erosion of a region of slight relief. The region was then raised, perhaps 400 or 500 feet, the sea retreated, subaerial denudation etched out the land surface, then moderate submergence took place to a level about 340 feet below the Cornwall level and the second or Goshen terrace was planed. Another oscillation resulting in a final elevation of 240 feet more permitted the sea to cut the Litchfield terrace. The Prospect terrace was cut when the region was raised about 220 feet further and perhaps after a prolonged stage of subaerial denudation. Lower and younger levels were cut by the waves on the seaward slopes before the sea reached its present position with reference to the land, each phase being represented by subaerial denudation farther inland. The sediment which had been spread over the sea bottom as the land was cut away to form the terraces has been removed by the streams since the successive terraces were subjected to subaerial erosion.

Twelve colleges and universities were represented by the fifty-two persons who were on the excursion.

The beauty of the autumnal coloring and of the region traversed added pleasure to one of the most interesting and largely attended of these annual excursions.

H. F. CLELAND,
*Secretary*

### THE WILLARD GIBBS PROFESSORSHIP OF RESEARCH IN PURE CHEMISTRY

In course of the academic year 1914–15, a new department was established on a permanent basis in the Mellon Institute of Industrial Research of the University of Pittsburgh, namely, a department of research in pure chemistry. The headship of this department is to be known as the Willard Gibbs Professorship of Research in Pure Chemistry, "to ever proclaim the ideal which the incumbents of the chair and the groups of research workers to be associated with them will be expected to follow." It constitutes a chair in the graduate school of the University of Pittsburgh as well as in the Mellon Institute.

Dr. Martin A. Rosanoff, who had built up and for years headed the graduate department of chemistry in Clark University, was formally inaugurated as the first permanent incumbent of this professorship, at exercises held in the Assembly Hall of the Mellon Institute on October 26. The exercises included addresses by Professors James McKeen Cattell and Marston Taylor Bogert, both of Columbia University, and by Dr. Rosanoff, followed by a reception and an inspection of the Mellon Institute.

Chancellor Samuel Black McCormick presided at the exercises. He explained the circumstances connected with the establishment of the chair and thanked the donors whose generosity had made possible the endowment, of which the income, amounting to $5,000 per annum, is to constitute the salary of the incumbent of the chair. He then introduced Dr. Cattell.

Dr. Cattell spoke on "The Support of Scientific Research in a Democracy."

His argument was that science with its applications had given us democratic institutions by so greatly increasing the productivity of labor and the length of life, that it had become possible to provide for education and equality of opportunity. But scientific research, unlike most services, can not be sold to individuals; it is for the benefit of all and must be paid for by all. Hitherto aristocratic institutions have been more favorable to scientific research than ours, but we are now beginning to equal the leading European nations in our contributions to the advancement of science. We must do much more in the future. Coal is mined in Pennsylvania to the value of some three hundred million dollars a year. In so far as natural resources are consumed, ten per cent. of the proceeds—thirty million dollars a year—might very properly be devoted to discovering new ways to obtain energy. The manufactures of the city of Pittsburgh and of Allegheny County are worth more than three hundred million dollars a year. These, like the proceeds of the mines, have been made possible by the applications of science. Ten per cent. of their value—another thirty million dollars a year—might to advantage be spent in this city for the further advancement of science under the auspices of the University of Pittsburgh. By the Bessemer steel process the world saves, according to the estimates of the late Abram S. Hewitt, each year two billion dollars; by the electro-magnet discovered by Faraday in the only research laboratory then existing, the world earns each year perhaps twice as much. Why should not these six billion dollars a year be set aside as a memorial to the scientific men and workers in technology who have provided the world with its vast wealth, to be used for the further increase of this wealth and for the welfare of all? We could afford to devote one tenth of all labor, one fourth of all wealth, to scientific work, and we should become each year a richer and a greater nation. We need a Mellon Institute for each science and in every city.

Following Dr. Cattell, Dr. Bogert, who was introduced by Dr. Raymond F. Bacon, director of the Mellon Institute, spoke on "The Especial Value of Research in Pure Chemistry."

After rapidly reviewing some of the more striking contributions which chemistry has made to civilization, he pointed out that it was research in pure science which laid the deep and abiding foundations upon which applied science has erected all the wonderful structure of modern industrial processes. The nineteenth century has been called the Age of Physics and Engineering, but the twentieth will surely be the Age of Chemistry.

Dr. Bogert referred to the great service

which the University of Pittsburgh and its Mellon Institute are now rendering the public, and congratulated them upon the establishing of the new chair of research in pure chemistry as one of the most effective ways of furthering research and a momentous step in the direction of building up a splendid new graduate school. He believed that a university's true greatness was not measured by the magnificence of its plant, the size or athletic prowess of its student body, but by excellence of its graduate school, and that the standard of the latter was determined by the amount and quality of its production of genuinely original work, and the character of its faculty. He said, further, that the extent of the assistance which a university secures from its surrounding community in the creation of such a fine graduate school as that of the University of Pittsburgh was often a good indication of the attitude of the university authorities themselves toward such work. In this respect he said that the University of Pittsburgh was, indeed, fortunate in having a chancellor and trustees who knew how to prize original scientific investigation at its real worth, and under whose fostering care and guidance it would have full opportunity to grow and flourish.

The designation of the new chair as the Willard Gibbs professorship of research in pure chemistry was peculiarly appropriate, and should prove a constant inspiration to all incumbents in their search for truth. Dr. Bogert also congratulated the chancellor and Director Bacon upon securing as the first occupant of the chair so distinguished a scientist as Dr. Rosanoff—chemist, physicist, mathematician and linguist—whose investigations, especially those on distillation and the catalysis of organic reactions, have already won for him an enviable international reputation. He then felicitated Dr. Rosanoff upon his appointment to a post of such conspicuous dignity and honor. In closing, the speaker called attention to the fact that it was the development of genius that was of greatest concern to the world, and that many of the investigations carried out by the Willard Gibbs professors would undoubtedly survive long after the beautiful buildings of the university had crumbled in decay.

The last of the addresses was a brief discourse on Willard Gibbs's life and work by Dr. Rosanoff. In the conclusion of his address, Dr. Rosanoff expressed the sentiment that a man whose intellectual achievement is as wonderful as that of Willard Gibbs need hardly be honored either by a monument in stone or bronze or even by so magnificent a memorial as a professorship of research endowed in perpetuity. The sense of attaching his illustrious name to the new chair is really the hitching of the wagon to a star. And Dr. Rosanoff solemnly pledged himself, ever guided by the light of that star, to humbly follow in the steps of the great American master of scientific research.

The Department of Research in Pure Chemistry of the Mellon Institute is to be conducted along the same lines as similar departments in the leading universities in this country and abroad. Graduates of colleges in good standing will be admitted to it as candidates for the master's and doctor's degrees, and it will be the principal duty of the head of the department to maintain in it the highest attainable academic standard. The industrial research fellows of the Mellon Institute and the members of the faculty of the school of chemistry of the University of Pittsburgh, as well as the members of the Federal Bureau of Mines and of Standards, are at liberty to attend all lectures, seminars, and other exercises conducted in the department. Further, the head of the new department promotes the industrial ends of the Mellon Institute by any scientific advice that he may be able to give in connection with the work of the industrial research fellows.[1]

The department of research in pure chemistry occupies a suite of rooms in the Mellon Institute building[2] and its members are at liberty to draw on the material facilities of the institute on the same terms as the members of

[1] On the object and work of the Mellon Institute, see *J. Ind. Eng. Chem.*, 7 (1915), 343.

[2] For a description of this building, see *J. Ind. Eng. Chem.*, 7 (1915), 333.

the industrial section. Those of the graduate students who are granted the title of fellow in pure chemistry are considered by the Mellon Institute as equal in rank and privileges with the industrial research fellows.

W. A. Hamor[3]

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## *FREDERIC WARD PUTNAM*

The following minute on the life and services of Professor Putnam was placed upon the records of the Faculty of Arts and Sciences of Harvard University at the meeting of October 26, 1915:

Frederic Ward Putnam, son of Ebenezer and Elizabeth Appleton Putnam, was born in Salem, Massachusetts, April 16, 1839. His ancestors on both sides were early immigrants from England, the first American ancestor being John Putnam, who settled in Salem in 1640. The father, grandfather and great-grandfather of Professor Putnam were all graduates of Harvard College, and the associations of his mother's family had been close with the institution from its beginning.

His early schooling was received in private schools and at home under his father's tuition. At an early age he showed great interest in natural history and had thoughts of devoting himself to scientific pursuits. Promised an appointment at West Point, however, he was preparing himself to adopt a military career, when Louis Agassiz met him at the Museum of the Essex Institute, and, recognizing in him a keen student, persuaded him to take up natural history as his life work. His preparation for the military profession was accordingly abandoned, and he devoted himself at once to the study of birds, being made curator of ornithology in the Essex Institute in 1856. In the following year he was made assistant to Professor Agassiz, and entering the Lawrence Scientific School received the degree of S.B. in 1862. For a number of years after this, he continued his work in the study of animal life, as curator of vertebrates at the Essex Institute, of Ichthyology at the Boston Society of Natural History, and as assistant in the museum of comparative zoology at Harvard University. During these years he had also some experience in museum administration, as he was entrusted with the charge of the Museum of the Essex Institute and of the Museum of the East Indian Marine Society in Salem, and later was made director of the museum of the Peabody Academy of Science in the same city. In 1873 he was chosen to fill the important position of permanent secretary of the American Association for the Advancement of Science, an office which he held for twenty-five years. Professor Putnam's connection with the association lasted through one of the most important periods in its life, and to his energy and administrative ability much of its success was due.

Although it was in the field of natural history that Professor Putnam for many years carried on most of his work, his interest in archeology was early aroused. While attending the meeting of the American Association at Montreal in 1857, he discovered on Mt. Royal a small kitchen-midden, and was thus among the very first in this country to recognize the presence of the remains of prehistoric man. In 1874 Dr. Jeffries Wyman, the first curator of the Peabody Museum of American Archeology and Ethnology, died and Professor Putnam was appointed to take the place. Thus began his connection with the institution of which he was the director for over forty years. In 1886, he was made Peabody professor of American archeology and ethnology, and served as such until 1909, when he joined the group of the emeriti. During the nearly half-century of his connection with the museum, he labored unceasingly to build up its collections, both by purchase and by explorations in the field. He was one of the earliest to realize the need of archeological and anthropological exploration, and to insist that careful field-notes and data are equally important with the specimens themselves. The great collections which he gathered and which place the museum in the forefront of the museums of this country, are a witness of his success.

With the appointment to the Peabody professorship, Harvard University became one of the first institutions in the country to offer instruction in the field of anthropology and American archeology. Although always more active in museum and field-work than in teaching, Professor Putnam strove constantly to develop the instruction in his chosen science, from the time when, before any courses were offered, he had several voluntary students working under his direction, to the very last days of his life, when nearly a score of specialized courses were offered by the division of which he had for so long been the head.

Striking as were the results of his work here at Harvard, his influence was literally nation wide, and he may justly be called one of the fathers of

[3] Assistant to the director of the Mellon Institute of Industrial Research.

anthropology in America, the others being Brinton, of Pennsylvania, and Powell, of Washington. In 1892, he was made chief of the department of anthropology at the World's Columbian Exposition at Chicago, and not only brought together what was probably the greatest archeological and anthropological exhibit ever gathered in this country up to that time, but collected by the aid of numerous collaborators a vast mass of data in regard to our native peoples which has not yet been fully worked up. The specimens so brought together for the Exposition became the nucleus of the present Field Museum of Natural History, which may thus be regarded as, in its origins, of his making. In 1894 Professor Putnam was made curator of anthropology in the American Museum of Natural History in New York City, and for nearly ten years he applied to its administration and to the building up of its collections the experience he had gained in Cambridge and Chicago. It was again largely through his efforts that the University of California established a department of anthropology in 1903, and he held the position of professor of anthropology and director of the anthropological museum in that institution until his retirement in 1909.

In his long life of scientific work, a great deal of Professor Putnam's attention was necessarily devoted to administrative and editorial duties, but he found time, nevertheless, for the writing of a large number of scientific papers, and the bibliography appended to the Anniversary volume presented to him in 1909 by his associates and students, contains something over four hundred titles. His membership in scientific societies, both at home and abroad, was extensive. In this country he was a member of the National Academy, the American Academy of Arts and Sciences, the American Philosophical Society, and a great many others; outside the United States he was an honorary or corresponding member of the anthropological societies of Great Britain, Paris, Stockholm, Berlin, Rome, Florence and Brussels, of the Royal Society of Edinburgh, and others of minor importance. In 1868 he received the degree of A.M. from Williams College; in 1894 the degree of S.D. from the University of Pennsylvania; and in 1896, the Cross of the Legion of Honor from the French government.

Professor Putnam was twice married: in 1864 to Adelaide Martha Edmands of Cambridge, by whom he had three children, two daughters and one son. After her death, fifteen years later, he married in 1882 Ester Orne Clarke, of Chicago, who, with his three children, survives him.

To his students and associates Professor Putnam endeared himself by his kindliness and sympathy. For every one he had a cordial greeting and a pleasant word. He was ready always to give unstintedly of his time, and so far as he was able, from his pocket, to any one who asked his aid. He had the gift of inspiring those with whom he came in contact with enthusiasm for research, and to few is it given to have so wide an influence. Until the very last years, the great majority of those in this country who were working in the anthropological field had been associated more or less closely with Professor Putnam, either as students or colleagues in the various institutions of which he had been a member. His loss, therefore, is widely felt, not only because he was an able administrator and the last of the three founders of anthropological study in this country, but also because he had won the affections of so many as a teacher and a friend.

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## *SCIENTIFIC NOTES AND NEWS*

PROFESSOR M. I. PUPIN, of Columbia University, will give a lecture on "The Problem of Aerial Transmission," at the approaching meeting of the National Academy of Sciences. The lecture will be given at the American Museum of Natural History, New York City, on the evening of November 15.

AT the recent meeting of the Clinical Congress of Surgeons of North America, at Boston, Dr. J. M. T. Finney, of the Johns Hopkins University, was elected president.

DR. RICHARD MILLS PEARCE, professor of research medicine in the University of Pennsylvania, received the degree of doctor of science from Lafayette College on October 20.

THE Geographical Society of Philadelphia presented on November 3 its Elisha Kent Kane medal to Professor Ellsworth Huntington, of Yale University. Dr. Huntington gave a lecture on "Climate, Weather and Daily Life."

ACCORDING to a cablegram to the daily press from Stockholm it has been decided to award the Nobel Prize in medicine for 1914 to Dr. Robert Barany, of the University of Vienna, for his work in the physiology and pathology of the ear.

The College of Physicians of Philadelphia announces that the Alvarenga prize for 1915 has been awarded Dr. Joshua E. Sweet, Philadelphia, for his essay entitled "The Surgery of the Pancreas."

M. Paul Painlevé, professor of mathematics in the University of Paris and professor of mechanics in the Paris Polytechnic School, has been made minister of education in the new French cabinet.

The French minister of war has appointed a consulting committee of experts on military aeronautics which includes M. Appell, who occupies one of the chairs of mechanics at the Sorbonne; M. Deslandres, director of the Meudon Observatory, and M. Kling, director of the Municipal Observatory.

Surgeon W. McCoy, now in charge of the leprosy investigations at the hospital at Molokai, Hawaii, has been appointed director of the hygienic laboratory of the U. S. Public Health Service, succeeding Dr. John F. Anderson, who recently resigned.

Dr. Sigismund S. Goldwater retired this week from the post of health commissioner of New York City and is succeeded by Dr. Haven Emerson, deputy health commissioner.

Mr. P. Baracchi has resigned as state astronomer of Victoria, and it is said that no appropriation will be made by the government for the Victorian Observatory at Melbourne.

Professor John Ferguson has retired from the regius chair of chemistry in the University of Glasgow, to which he was appointed in 1874.

Professor Pollitzer, of Vienna, the distinguished otologist, has celebrated his eightieth birthday.

Professor Ernst Neumann, formerly director of the pathologic institute in Königsberg, and a well-known authority on diseases of the blood, celebrated the sixtieth anniversary of his doctorate on September 24.

D. J. Guzman has been appointed director of the Institute of Vegetal Pathology in Salvador, Central America, as organized under the ministry of agriculture of that government.

Dr. J. S. Caldwell, plant physiologist of the Alabama Experiment Station, has been appointed to a position as specialist in fruit by-products investigations at the Washington Experiment Station, State College of Washington, Pullman.

Dr. J. N. Rose, research associate of the Carnegie Institution of Washington, has returned from a five months' exploration trip along the east coast of South America, where he made extensive botanical collections, especially of cacti, ferns and mosses. His collection consists of herbarium, formalin and greenhouse material, supplemented by numerous photographs.

Another expedition to Japan and Korea has been undertaken by Associate Professor Frederick Starr, of the University of Chicago, who sailed from Tacoma by the *Mexico Maru* on October 7. His photographer and helper on the expedition will probably be Hambei Maebashi, of Tokyo. The expedition is planned to cover six months' time, Professor Starr expecting to return to the university in time for the opening of the spring quarter in April, 1916.

Professor Henri T. Hus, of the botanical department of the University of Michigan, has been granted a three years' leave of absence from the university, beginning with the present college year, in order to enable him to accept a position with the United States Rubber Company in Sumatra. Professor Hus will take charge of experimental plant breeding work on the island.

The expedition under Captain Vilkitski in the ice-breakers *Taimur* and *Vaigach*, which last year started from Vladivostok to renew the attempt to reach Europe by the Arctic Sea north of Asia, and which wintered in 100° E. longitude off the Taimur peninsula, reached Archangel during September, having thus for the first time accomplished the complete northeast passage in the reverse direction from that folowed by the *Vegg*.

We learn from *Nature* that the council of the Chemical Society, London, has arranged for three lectures to be delivered at the ordinary scientific meetings during the coming

session. The first of these lectures will be delivered on November 18, by Dr. E. J. Russell, who has chosen as his subject, "The Principles of Crop Production." The titles of the two later lectures to be delivered on February 3 and May 18, by Professor W. H. Bragg and Professor F. Gowland Hopkins, respectively, will be announced later.

THE annual commemoration of the benefactors of the Royal College of Physicians of London took place on October 18, when the Harveian Oration was delivered by Dr. Sidney Coupland. The usual dinner was not held.

A GRANITE boulder has been placed on the campus of Cornell University to serve as a memorial of the late Ralph Stockman Tarr, professor of geology and physical geography, 1892–1912. The stone stands almost under one of the windows of the lecture room in McGraw Hall where Professor Tarr met his classes for many years. A bronze memorial tablet will be set into the eastern side of the boulder. The tablet has been designed by Mr. H. S. Gutsell, of the college of architecture. It will bear a portrait bust in low relief and the inscription:

RALPH STOCKMAN TARR
1864–1912
Scientist—Writer—Teacher

This boulder, a relic of the ice age, symbolic of his research in glacial geology and of the enduring value of his work, is placed here as a memorial of their friend and adviser by his students.

NATHAN FREDERICK MERRILL, professor emeritus in the University of Vermont, died on October 26 at the age of sixty-six years. He was for thirty years professor of chemistry in the University of Vermont.

ANDREW FERRARI, a chemist known for research work in glass manufacture, died at Hartford, Conn., on October 26, aged seventy-six years.

ALICE GERTRUDE MCCLOSKEY, A.B. (Cornell, '08), assistant professor of rural education in the college of agriculture, Cornell University, died on October 19.

DR. JOHN MEDLEY WOOD, director of the Natal Herbarium and formerly director of the Natal Botanic Garden, died on August 25, at the age of eighty-six years.

MAJOR A. J. N. TREMEARNE, an Australian by birth, known for his publications on the anthropology of the African races, was killed in Flanders on September 25.

PROFESSOR SCHMIDT-RIMPLER, formerly ordinary professor of ophthalmology, died at Halle, on September 23, aged seventy-seven years.

DR. TIEFENBACHER, emeritus professor of railway engineering in the Vienna School of Technology, has died at the age of seventy-two years.

ACCORDING to *Nature* the third annual meeting of the Indian Science Congress will be held in Allahabad from January 13–15, 1916, when Sir Sidney Burrard, F.R.S., will be president. The chief sections will be physics, chemistry, zoology, botany, agriculture and ethnology, and the presidents of the respective sections Dr. Simpson, of the Meteorological Department; Dr. Sudborough, of the Research Institute, Bangalore; Dr. Woodland, of Allahabad; Dr. Howard, of Pusa; Mr. Coventry, of Pusa; and Mr. Burn. It is hoped that the local committee will persuade Dr. Bose to give a public lecture on his own researches. The local secretaries for this year are Dr. Hill, of Muir College, and Mr. P. S. Macmahon, of the Canning College, Lucknow, to the latter of whom all communications should be addressed. The congress is under the general control of the Asiatic Society of Bengal.

LEADING Philadelphia physicians are working out a plan to make Philadelphia a great center of advanced research and study in medicine. More than one hundred physicians have joined the Cooperative Association for Post-graduate Teaching of Medicine, which is the temporary organization to bring about this advance in Philadelphia's medical life. Dr. David Reisman, 1715 Spruce Street, is chairman, and Dr. George P. Muller, 29 Pine Street, is temporary secretary. J. M. Anders is chairman of the executive committee, on which are the following physicians: W. M. L. Coplin, F. X. Dercum, P. B. Hawke, Edward Martin, Charles K. Mills, George P. Muller, R. M.

Pearce, David Reisman, W. L. Rodman and George E. de Schweinitz. The subcommittee on roster includes Ward Brinton, R. Max Goepp, F. C. Hammond, John A. Kolmer, R. V. Patterson, C. M. Purnell, W. J. Taylor, T. H. Weisenburg, A. D. Whiting and Samuel Woody. A central bureau with a permanent secretary is to be established. The preliminary work to be done includes tabulating the lecture courses, laboratory hours, hospital and dispensary hours and getting the cooperation of all medical schools and hospitals in the city so their doors will be open to the post-graduate student.

At the annual meeting of the National Association of Audubon Societies held recently in the American Museum of Natural History there was an exhibition of motion pictures by Mr. Herbert K. Job, who was sent by the association on a tour of inspection of the bird reservations in Florida and Louisiana.

In a report of the clay-working industry of the United States in 1914 by Jefferson Middleton, issued by the United States Geological Survey, the value of the clay products of this country in 1914 are given as $164,986,983—brick and tile $129,588,822 and pottery $35,398,161. This was a considerable decrease compared with 1913, but as compared with the value for the last twenty years it shows a great growth in the industries. Compared with 1908 there was an increase of $31,789,221. In only four years—1909, 1910, 1912 and 1913—has the total value of clay products exceeded that of 1914. Considered by the average for five-year periods, which is perhaps the fairest comparison, as unusual conditions may occur in a single year, the average annual value of the clay products of the United States was: 1895–1899, $72,233,056; 1900–1904, $118,135,826; 1905–1909, $153,838,231, and 1910–1914, $170,287,909. It will be seen that the value of the clay products of the country has considerably more than doubled in the last twenty years. With the revival of business, which is clearly indicated, the great clay-working industries, with the inherent superiority of their products for many uses, are bound to come into their own, the halt of 1914 being but a temporary setback which will be more than overcome in the near future. Clay products are made in every state. Of the territories, Alaska and Hawaii reported none. The value of clay products ranged in 1914 from $5,974 in Porto Rico to $37,166,768 in Ohio. Ohio reported over one fifth of the value of clay products in 1914 and has been the leading state since figures on this subject were first compiled by the Geological Survey in 1894. It is likely to maintain this position, as its output has always greatly exceeded that of the second state, Pennsylvania, and in 1914 this excess was $15,319,772, or over 70 per cent. Pennsylvania's output in 1914 was valued at $21,846,996, or over one eighth of the total for the United States. New Jersey ranked third in 1914, with products valued at $16,484,652. Illinois was fourth, with products valued at $13,318,953, and New York was fifth, with products valued at $9,078,933. Indiana was sixth; Iowa was seventh, exchanging places with Missouri, which was eighth; West Virginia was ninth, exchanging places with California, which was tenth. The first ten states reported wares valued at $128,253,688, or 72.74 per cent. of the total. The first five states reported wares valued at $97,896,302, or nearly 60 per cent. of the total.

## *UNIVERSITY AND EDUCATIONAL NEWS*

Mr. James J. Hill has presented $125,000 to Harvard University to be added to the principal of the professorship in the Harvard graduate school of business administration, which bears his name. The James J. Hill professorship of transportation was founded by a gift of $125,000, announced last commencement day, the donors including John Pierpont Morgan, Thomas W. Lamont, Robert Bacon and Howard Elliott.

The General Education Board announces that $100,000 has been given to Carlton College, Northfield, Minn., toward a fund of $400,000; $50,000 to Hobart College, Geneva, N. Y., toward a fund of $200,000; $200,000 to Lafayette College, Easton, Pa., toward a fund of $1,000,000, and $25,000 to Kalamazoo Col-

lege, Kalamazoo, Mich., toward a fund of $100,000.

THE trustees of the Joseph Bonnheim Memorial Fund, founded in 1897 by Albert Bonnheim and Fannie Bonnheim, of Sacramento, in memory of their son, have conveyed the entire property of the trust, now valued at approximately $100,000, to the University of California. The income of the endowment will be devoted to scholarships in the University of California for young men and young women.

CONSTRUCTION is about to begin on a laboratory building, to cost $100,000, to be erected by the University of California on the new 465-acre site just purchased by the University of California, at a cost of $55,000, for its citrus experiment station and graduate school of tropical agriculture at Riverside. The director of this work of agricultural research at Riverside is Dr. Herbert J. Webber, former professor of plant breeding in Cornell University.

WORK has begun on the foundation for the five-story building of the Hunterian Laboratory connected with Johns Hopkins Medical School. The new building is located at the corner of Wolfe and Madison Streets, will be 50 by 100 feet and will cost about $65,000.

NEW YORK UNIVERSITY has added to its graduate school courses in surgery by which it will be possible for graduate students to secure the advanced academic degrees of master and doctor of science. The course does not deal with the technique of surgical practise but with subjects such as the application of biological science to surgical diagnosis and therapy.

MR. JAMES COLE ROBERTS, of the United States Bureau of Mines, Denver, has been appointed to the Joseph Austin Holmes professorship of safety and efficiency engineering in the Colorado State School of Mines.

DR. ROBERT S. MORRIS, formerly of the Johns Hopkins University, has been appointed to the Frederick Forchheimer chair of medicine in the medical department of the University of Cincinnati.

DR. ROBERT H. MULLIN, director of the laboratories of the Minnesota State Board of Health and assistant professor of pathology and bacteriology at the University of Minnesota, has accepted an offer from the University of Nevada, at Reno, to take charge of the hygienic laboratories of that institution.

DR. H. G. EARLE has been appointed professor of physiology at the University of Hong Kong.

DR. HERMANN JORDAN, docent in Tübingen, has been called as associate professor of comparative physiology in Utrecht, as successor to the late Professor A. A. W. Hubrecht.

PROFESSOR BENECKE, of the Berlin Agricultural School, has been called to the chair of botany at Münster, as successor to Professor Correns.

DR. BORIS ZARNIK, associate professor at Würzburg, has accepted the professorship of zoology at the University of Constantinople.

## *DISCUSSION AND CORRESPONDENCE*

### ELECTROMOTIVE PHENOMENA AND MEMBRANE PERMEABILITY

IN his very interesting presidential address, printed in SCIENCE, Professor Bayliss[1] discusses among other things the origin of electromotive forces in living cells. In this discussion Professor Bayliss adopts the theory, originally suggested by Ostwald and elaborated by Bernstein, R. Lillie and Höber, that the E.M.F. observed in living tissue is due to a selective ion permeability in the sense that normally only cations are able to diffuse through the membrane, but that if the membrane is injured or if a cell is active its membrane becomes also permeable for anions. As a consequence of this increase in permeability the spot where this happens must become negative if compared with a spot of normal or resting tissue. To quote Professor Bayliss:

> I referred previously to the electrical change in excitable tissues and its relation to the cell membrane. It was, I believe, first pointed out by Ostwald and confirmed by many subsequent investigators, that in order that a membrane may be impermeable to a salt it is not a necessary condition

[1] SCIENCE, 1915, N. S., XLII., No. 1085, p. 509.

that it shall be impermeable to both the ions into which this salt is electrolytically dissociated. If impermeable to one only of these ions, the other, diffusible, ion can not pass out beyond the point at which the osmotic pressure due to its kinetic energy balances the electrostatic attraction of the oppositely charged ion, which is imprisoned. There is a Helmholtz double layer formed at the membrane, the outside having a charge of the sign of the diffusible ions, the inside that of the other ions. Now, suppose that we lead off from two places on the surface of a cell having a membrane with such properties to some instrument capable of detecting differences of electrical potential. It will be clear that we shall obtain no indication of the presence of the electrical charge, because the two points are equipotential, and we can not get at the interior of the cell without destroying its structure. But if excitation means increased permeability, the double layer will disappear at an excited spot, owing to indiscriminate mixing of both kinds of ions, and we are then practically leading off from the interior of the cell, that is, from the internal component of the double layer, while the unexcited spot is still led off from the outer component. The two contacts are no longer equipotential. Since we find experimentally that a point at rest is electrically positive to an excited one, the outer component must be positive, or the membrane is permeable to certain cations, impermeable to the corresponding anions. Any action on the cell such as would make the membrane permeable, injury, certain chemical agents, and so on, would have the same effect as the state of excitation. If we may assume the possibility of degrees of permeability, the state of inhibition might be produced by *decrease* of permeability of the membrane of a cell, which was previously in a state of excitation owing to some influence inherent in the cell itself or coming from the outside. This manner of accounting for the electromotive changes in cells is practically the same as that given by Bernstein.

The suggestion of Ostwald was questioned by physical chemists, *e. g.*, Walden, Tammann and Nernst. Recent experiments carried on in the writer's laboratory have shown that the attempt to explain the E.M.F. in tissues by the idea of a selective ion permeability and its changes (which the writer had originally also adopted) is neither tenable nor necessary. Space permits to point out only a few of the reasons for this statement.

1. Loeb and Beutner found that if we lead off from two places on the surface of an intact plant leaf (*e. g.*, rubber plant) or fruit (*e. g.*, apple) with two solutions of the same electrolyte but of different concentration, the lower concentration is always positive to the higher; and the E.M.F. depends upon the ratio of the two concentrations as expressed by Nernst's well-known formula. In the most ideal objects for this purpose the E.M.F. corresponds quantitatively to Nernst's formula. In all cases a spot of tissue (no matter whether plant or animal) in contact with distilled water is positive if compared with a spot in contact with a physiological salt solution or a Ringer solution.[2]

According to the theory of Bernstein, which Bayliss adopts, a spot of muscle or leaf in contact with distilled water should be negative to a spot in contact with a physiological salt solution, since we know that distilled water causes an increase in permeability. This increase in permeability is shown not only by the facts of cytolysis, but also by direct observations on the eggs of *Fundulus* in the writer's floating experiments. Thus one of the most general phenomena in electrophysiology contradicts the theory of selective ion permeability.

The experiments of Beutner and of Loeb and Beutner have shown that the E.M.F. which appear at the surface of living tissues can be imitated if we bring a watery salt solution in contact with a substance immiscible in water, such as lecithin or oleic acid (which for experimental purposes was dissolved in guaiacol).[3] According to Beutner's theory[4] traces of the salts are soluble in the water immiscible phase and one ion combines here with an anion or cation (or both combine in the case of an amphoteric electrolyte). The common ion of the salt in the water and of the water immis-

[2] Loeb, J., and Beutner, R., *Biochem. Ztschr.*, 1912, XLI., p. 1.

[3] Loeb and Beutner, *Biochem. Ztschr.*, 1913, LI., p. 288; 1914, LIX., p. 195.

[4] Beutner, *Ztschr. f. physik. Chem.*, 1914, LXXXVII., p. 385; *Jour. Am. Chem. Soc.*, 1914, XXXVI., pp. 2,040 and 2,045.

cible salt determines the E.M.F. On the basis of this assumption Beutner has been able to explain the observed phenomena thermodynamically, and his theory accounts for all E.M.F. at phase boundaries.

It follows from this theory that a spot of tissue in contact with distilled water should be positive to one in contact with a physiological salt solution, which is actually the case.

It was known that if we lead off from two spots of a muscle with a KCl and NaCl solution of the same concentration, the spot in contact with KCl appears negative to that in contact with NaCl. Loeb and Beutner could show that this was also true for the boundary between oleic acid or lecithin and salt solutions, and it was explained by Beutner on the assumption (which could be verified) that KCl is more soluble in the water immiscible phase than NaCl, that the Cl combines with some constituent of the water immiscible phase, and that the Cl ion, being the common ion, determines the E.M.F.

2. It is well known that an injured spot of a tissue is negative to a non-injured spot (current of injury), and this is explained on the basis of Bernstein's or Bayliss's theory by assuming that the specific cation permeability of the membrane is abolished by the injury and that the anions will also reach the outer boundary of the injured spot. Loeb and Beutner have published experiments which show that the so-called current of injury has nothing to do with an alteration of permeability, but that it is in all probability due to the existence of an internal E.M.F. situated at the internal boundary of the membrane.[5] If it is true that the junction of the outer boundary of an organ, *e. g.*, an apple, with a salt solution must become the seat of an E.M.F., the same must be true for the boundary of the membrane with the internal liquids of the tissues containing electrolytes. If we could show that this internal E.M.F. is generally smaller than the E.M.F. at the outside surface when we lead off from the latter with a physiological salt solution, the so-called current of injury would find a simple explanation. As a matter of fact, acids, KCl, and other substances give rise to negative potentials if compared with that of a physiological salt solution of the same concentration, and it is quite possible that such a solution exists at the inner boundary of the membrane. When we lead off from two intact spots on the surface of an organ we do not notice the existence of the internal potentials, since they are opposite and equal; but if we destroy the membrane on one spot and lead off from this spot and from an intact spot of the skin the injured spot must be negative to the normal spot, since in reality we measure in this case the difference between the outer and the inner potentials of the membrane. The idea that such a layer exists at the internal surface has received support by a series of experiments by Loeb and Beutner, some of which may be mentioned. When we cut an apple in two and lead off with salt solutions of the same kind and concentration from the intact outer surface and from the cut, the cut is negative, as was to be expected. When we remove more and more of the flesh of the apple, while leaving the skin unaltered, the difference of potential between outside skin and cut surface at first remains unchanged; but if we remove so much that the salt solution (with which we lead off from the cut surface to the measuring instrument) reaches the internal surface of the rind, the E.M.F. between the intact and injured part of the apple becomes less and finally disappears. In this case the salt solution replaces, in our opinion, the natural layer of liquid on the inner surface of the skin.

When we press the skin of an apple on one spot with the soft part of our finger without causing an abrasion of the skin, that spot becomes negative to a non-pressed spot; and yet we can show that the permeability of the skin is intact. This can be shown by the fact that the concentration effects produced by applying solutions of different concentrations are still the same as in any intact part of the skin; while this is no longer the case if we cause an abrasion. The explanation which we ventured to give for the fact that the pressed spot be-

[5] Loeb and Beutner, *Biochem. Ztschr.*, 1912, XLIV., p. 304.

comes negative was that the pressure displaces the layer of acid or any other substance which may be responsible for the inner potential, and replaces this substance by the juice pressed out from some of the soft cells of the flesh of the apple, but without altering the permeability of the skin.

Under these conditions the fact that the active part of a tissue becomes negative to a part at rest finds its simple explanation on the assumption that in the active part of the tissue substances are formed which temporarily alter the potential at the inner surface in such a sense as to make the outside on that spot appear more negative. There is no necessity for assuming any increase in the permeability of the skin.

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## What is Hellenism?

It would usually be both foolish and ungrateful to criticize the choice of illustrations used by a lecturer in an attempt to make clearer a worthy proposition. When, however, the illustration is in very common use, though more often in another manner, when it is sure to remain in common use, and when moreover it has great and positive value, its misuse is dangerous enough to merit attention. The address of Professor Harrison, published in Science of October 23, 1914, urges us to make science of practical value. We may all well do what we can to share and spread the motive and accomplish its aim. In this address, Professor Harrison contrasts Hellenism and Hebraism, apparently in the sense that Hellenism typifies clear thought, and Hebraism vigor in practise. We are all familiar with the contrast of Greek and Hebrew culture, in which the former represents reason, and the latter faith, as the guiding principle of conduct. Professor Harrison's contrast strikes me as both novel and unsound.

The Hellenic culture which has influenced subsequent civilization was essentially the culture of Athens. The usual idea of Athenian culture is that it was characterized by marvellous activity. As to the culture typical of Athens, we can go back to the greatest Greek historian, and as to Greek ideals, to the greatest Greek philosopher, both of them men whose works are still commonly regarded as preeminent in their fields. The opinion of Thucydides with regard to the Athenians is expressed over and over. In Chapter III. of Book I., he puts his views into the mouth of the envoy of Corinth, who is addressing an assembly in Sparta:

> The Athenians are addicted to innovation, and their designs are characterized by swiftness alike in conception and execution; you have a genius for keeping what you have got, accompanied by a total want of invention, and when forced to act you never go far enough. Again, they are adventurous beyond their judgment, and in danger they are sanguine; your wont is to attempt less than is justified by your power, to mistrust even what is sanctioned by your judgment, and to fancy that from danger there is no release. Further, there is promptitude on their side against procrastination on yours; they are never at home, you are never from it: for they hope by their absence to extend their acquisitions, you fear by your advance to endanger what you have left behind. They are swift to follow up a success, and slow to recoil from a reverse. Their bodies they spend ungrudgingly in their country's cause; their intellect they jealously husband to be employed in her service. A scheme unexecuted is with them a positive loss, a successful enterprise a comparative failure. The deficiency created by the miscarriage of an undertaking is soon filled up by fresh hopes; for they alone are enabled to call a thing hoped for a thing got, by the speed with which they act upon their resolutions. Thus they toil on in trouble and danger all the days of their life, with little opportunity for enjoying, being ever engaged in getting: their only idea of a holiday is to do what the occasion demands, and to them laborious occupation is less of a misfortune than the peace of a quiet life. To describe their character in word, one might truly say that they were born into the world to take no rest themselves and to give none to others.

The ethics of Aristotle represents happiness as the goal of human effort, and work as absolutely indispensable to happiness. No single quotation would give an adequate idea of the

insistency with which the importance of activity is emphasized. Even where amusement is recognized as sometimes worth while, and rest as sometimes necessary they are countenanced only because constant work is, in experience, impossible.

Rest, therefore, is not an end, because it is adopted with a view to working afterwards.

Happiness itself is repeatedly defined as "a working in the way of excellence." When Aristotle finds the highest happiness in intellectual contemplation, he explicitly justifies himself on the ground that intellectual contemplation is itself the exercise of the highest of human faculties, of that of the mind. Aristotle hardly pauses upon this point, before he goes forward to point out that the thoroughly wise man must proceed, if he would achieve all the happiness within his reach by making his wisdom effective, to do exactly what Professor Harrison is urging, to interest himself in public affairs, and thus find for his wisdom the greatest possible usefulness. The politics and ethics of Aristotle are tied together by this dependence of the highest happiness of the truly wise man upon public activity.

With the Greek historian and the Greek moulder of the world's thought on record as they are, it would be superfluous to quote, as might be done, from many other Greeks; and Greek history is too generally familiar to make it worth while to refer to the wealth of Greek achievement. As the histories of the two people are usually read, Hebrew culture and history were, in comparison with the Greek, nothing but an almost unbroken oriental slumber. The single great Hebrew achievement was the enunciation of faith.

Greek ideals are constantly urged, and Greek examples constantly held before us. If we were to let ourselves imagine that the acceptance of these ideals and these examples involve any kind of inactivity, it would be a calamity. The trend of human views and human ideals has for a long time been away from the Hebrew, and toward the Hellenic. It is not Hebraism which is just now "exuberant." There is rather too little willingness in the world to-day to trust any light, if its source lies beyond our reach. But it is a most imperfect form of Hellenism which is "exuberant." Greek activity was intellectual enough to keep its aims, even the most ultimate, fairly well in view. A very large part of modern activity is so blind to any aims, except the most immediate, that it has no means of testing the validity and worthiness of even such aims as are within its vision. Greek thought kept happiness in view as the goal of effort, and examined this goal with such care that Greek opinion concerning it is very generally held to-day by those who are familiar with Greek opinion. Modern thought has added amazingly little to Greek views.

Knowledge has been making amazing strides. But aside from medicine, modern knowledge contributes infinitely less than it should to the attainment of the ultimate goal. I do not know that the world was happier at any past time than it is now, but am very sure that there is very far from being the happiness now that there ought to be. The advance in knowledge during the past half century has not been accompanied by any corresponding development of happiness. Indeed, we do need to exert ourselves to make our knowledge worth while. We should study to understand what happiness is, and how we can make our activities effective in achieving it. When we do this, each in such measure as he can, we shall act according to the best Hellenic ideals, ideals not merely held by the Greeks, but expressed by them more perfectly, I believe, than has ever been done since.

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## UNIVERSITIES AND UNPREPAREDNESS

THE majority of persons are so absorbed in the events of the European war that little attention is paid to the consideration of methods by which this nation could coordinate and use its intellectual resources to the best advantage in making some positive contribution towards the rehabilitation of civilization. We should expect the universities to be keenly alive to the necessity for supplying the leaders of

thought essential in preparing the way for a new era, but these institutions are seriously hampered by a narrow provincialism and are generally quite willing to sacrifice national interests to the interests of their own alumni.

The following letter has been written in the hope that those who read it may be induced to express their views upon the methods available for curing our universities of that infirmity of spirit which is a symptom of the national malady of general unpreparedness for either war or peace.

PRINCETON, N. J., October 1, 1915.

*Dear Sir:* The present crisis in civilization has brought this country face to face with many new and grave responsibilities. We have suddenly awakened to an increased sense of appreciation of the need of adequate protection against invasion, of greater facilities for insuring the scientific development and extension of industry and commerce, of promoting research and scholarship, and of eventually developing a culture which will contain dynamic power sufficient to hasten the spread of the spirit of malice towards none and charity for all.

The people of the United States are now vaguely considering the possibility of making a contribution of permanent value to the cause of civilization, but substantial progress in this direction can only be gained under other standards of leadership, and by the dissemination of higher ideals than those hitherto exhibited by older civilizations. The task is an enormous one. Advance towards a newer civilization will tax human intelligence to the uttermost.

What active preparation are our universities making to assume their share in this great movement?

An extraordinary opportunity exists—one rich in possibilities, not only for coordinating but for strengthening the intellectual forces of the nation. World-problems must be solved in a world-spirit. Is not this the moment to break away from the narrow provincialism which interferes with the active participation of our universities in the general advance?

This provincialism is manifested in the form of administration of university affairs which allows the imposition of ideals entertained by those alumni who appraise the value of their alma mater in terms of sentimental attachments without considering the relation of the institution to the nation and to the intellectual life of the people.

For some years I have been trying to analyze conditions which seem to handicap the universities seriously in their effort to stimulate and direct the thought of the nation. In this connection I should consider it a favor if you would reply to the following questions:

1. Can you suggest a method by which a freer interchange of opinion and criticism between universities might be effected?

2. How can we combat the obsessions and overvalued ideas that are the common accompaniments of emotional reactions associated with athletic contests and which make it extremely difficult to substitute the national for the provincial ideal in university administration?

3. Do you believe that a broader and more intelligent spirit would be introduced into the administration of affairs if the principle was carried into practise of adding faculty representatives, including those from other universities, to each board of trustees or overseers?

4. Have you any suggestion to offer in regard to the changes in the present form of organization so that the administration of the finances and the formulation of the educational policy should not be under the control of a single board of trustees?

Thanking you for the courtesy of a reply, I am,

Respectfully,

STEWART PATON

PRINCETON, N. J.

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## SCIENTIFIC BOOKS

*The Mutation Factor in Evolution with Particular Reference to Œnothera.* By R. RUGGLES GATES, Ph.D., F.L.S. London, 1915. Macmillan and Co. Pp. xiv + 353. Price $3.25.

Dr. Gates has been a prolific contributor to the already very extensive literature on œnotheras, and this book will be a welcome summary of his views as expressed and modified through a long series of papers.

Following the "Introduction" is a chapter, accompanied by a map, on the "Character and Distribution of the Œnotheras," in which structural features and life habits are described and a list of twenty-eight species given with synonymy and accounts of their distribution. The fact that twelve species are appended to this list without discussion indicates how rapid is the progress being made, chiefly through the studies of Bartlett, to our knowledge of the

*Œnothera* taxonomy and how circumscribed is our present horizon. Every years adds some new species which have been tested in the experimental garden, the only safe way of determining for most of the œnotheras their true characters and justifying the publication of species. With these conditions in the group some years must elapse before we shall be in a position to take a general survey of the field.

A chapter on "The Cultural History of Œnothera" presents an interesting account of the pre-Linnæan references to œnotheras with a number of figures reproduced from old works. This account leads up to a discussion of the origin and status of *Œnothera Lamarckiana*, a matter of fundamental significance in any consideration of the value of this plant as representative of a mutating species. Gates accepts without qualifications the opinion of De Vries that *Lamarckiana* was collected in America by André Michaux about the end of the eighteenth century. The evidence for this view rests on a specimen in the Muséum d'Histoire Naturelle at Paris. The photograph of this plant published by De Vries and only figured in part by Gates shows a plant with narrow-lanceolate leaves strongly petioled, narrow bracts, and very long sepal tips. That this plant could have been related to *Lamarckiana* with its ovate-lanceolate leaves almost sessile, broader bracts, and shorter sepal tips seems to me scarcely possible, and I venture to believe that neither Bartlett nor Shull, both *Œnothera* specialists, will follow De Vries in this identification.

Accepting this identification of De Vries, Gates finds no difficulty in believing that *Lamarckiana*, as a native American species, established itself on the Lancashire coast of England previous to 1800, and that the figure of Sowerby, 1806, is of this plant in spite of the fact that the flowers as drawn are not so large as those of *Lamarckiana*, the position of the stigma not so high, and that no mention is made in the description of the very conspicuous red papillæ upon the stem. An alternative possibility that the plant of Sowerby was a form related to *biennis* finds no favor with Gates. The chapter concludes with accounts of a number of races of *Lamarckiana* now in cultivation or otherwise recognized. Gates accepts with De Vries the suggestion that the source of the *Lamarckiana* introduced into cultivation by Carter and Company about 1860 may have been not Texas, as they state, but England.

Chapters IV. and V. give descriptions of the "mutation phenomena" in *Lamarckiana* and other species and include observations of his own as well as those of De Vries and other authors. There is also described in these chapters the results of many breeding experiments involving the "mutants" as parents. These chapters should be read with the following points in mind as reservations of prime importance for judgment on the deductions. Recent work has shown that the germination of very many seeds of œnotheras is usually delayed far beyond the time generally allowed by the preservation of seed pans. Although the facts of delayed germination and seed sterility have been recognized, few investigators have taken the trouble to make counts of the seeds sown and until recently none have obtained complete germination as established by experimental methods properly checked through the examination of the residue of sterile seed-like structures. Consequently we can not feel confident that the records of any cultures of *Œnothera* so far reported are complete for their possible progeny. The percentages calculated for "mutants" and the ratios of classes in breeding experiments can not be accepted as final in exact genetical work. We are not in a position even to guess what may be the changes of front when exact data become available.

In Chapter VI., on the "Cytological Basis of the Mutation Phenomena," will be found an account of Gates's own contributions in cytology which have been noteworthy. A good description of the reduction divisions in the pollen mother-cells paves the way for the discussion of irregularities in the distribution of chromosomes whereby gametic nuclei may be formed with more or with fewer chromosomes than 7, the normal number. From such gametes, fertile zygotes are occasionally formed that give rise to œnotheras with high counts of chromo-

somes of which the *lata* and *semi-lata* types with 15, the *semi-gigas* with 21 and the *gigas* with 28 chromosomes, respectively, are the best known. Gates gives a clear account of his important studies on *lata* and comprehensive discussions of triploidy and tetraploidy, the latter with a long list of genera with species in which the chromosome number has been doubled. It is not likely that there will be disagreement with Gates's chief conclusion that the characters of these plants are correlated to some extent with the peculiarities of their chromosome counts, which are above the normal 14, and that some of the variants from *Lamarckiana* and other species, called "mutations" are due to irregularities in the distribution of their chromosomes.

However, certain features of the chromosome behavior at the time of reduction in the species studied by Gates are not discussed from all points of view. Gates emphasizes the fact that in the œnotheras studied by him the pairing of the chromosomes previous to the reduction division is very loose, so that mechanical arrangements favor the irregularities of distribution which actually occur. In these observations other students of *Œnothera* cytology agree for *Lamarckiana* and some of its "mutants" and for certain other forms. But Gates assumes that this peculiarity, *i. e.*, a loose pairing of chromosomes, is a condition prevalent among œnotheras in general, including those which he believes to be pure species, and that the forms of this genus depart from the rule, supported by an overwhelming mass of evidence, that in pure species there is a very exact pairing of chromosomes previous to the reduction divisions. Studies among such animals and plants as have a series of chromosomes of different sizes and forms have shown a most remarkable association of these in strictly homologous pairs, and the presumption is justified that a pairing of homologous chromosomes at the time of reduction is the rule in pure species.

One *Œnothera* is known, namely, a line of *grandiflora,* in which the chromosomes following synapsis become associated very definitely into ring-shaped pairs which are assembled in a very orderly manner at the equatorial plate of the reduction division and are subsequently distributed in equal numbers by the separation of the members of each pair. This is a history entirely in accord with that generally reported for animals and plants and the conditions allow of little or no opportunity for irregularities of chromosome distribution. Now it also happens that the seeds of this line as recently tested are almost wholly fertile and that there is scarcely more than a trace of pollen sterility. In short, this line with respect to its fertility and to the regularity of its reduction process presents the behavior to be expected of a pure species. Since one line of *Œnothera* is regular in its behavior, there are probably other lines equally so, and these may prove to be the species purest of all the œnotheras in their genetical constitutions. They have not yet been studied either cytologically or through breeding experiments. On the other hand, *Lamarckiana* and certain of its derivatives, types of very low seed and pollen fertility, irregular in their reduction divisions, and extraordinary in their breeding habits, have been given much attention and assumed by the mutationists to be representative of pure species. In the face of these exceptional peculiarities the natural assumption until otherwise disproved should be that *Lamarckiana* and these derivatives are not pure species and that the irregularities of their cytological and breeding habits are the result of a hybrid constitution. This is a phase of the matter which Gates does not consider.

A long chapter on "Hybridization" and one on the "Relation between Hybridization and Mutation" discuss the nature of mutation crosses, Mendelian splitting, blending and modification of characters, twin hybrids, double reciprocal crosses, etc. There is summarized here the most important of Gates's breeding studies, including those between *grandiflora* and *rubricalyx*. So far as the statements of ratios and percentages are concerned, the criticism of course applies here as to all past *Œnothera* work that we have no assurance of complete results since there is no means of knowing what proportion of the seeds sown was fertile or how imperfectly representative

may have been the cultures due to incomplete germination. One can not feel confident that the results were more than glimpses of the genetical possibilities and under these conditions speculation loses its point. We are not likely to be in a position to discuss satisfactorily the problems of *Œnothera* genetics until new series of experiments are undertaken with methods whereby the germination of seeds is forced to a finish.

Gates makes an attack on the extreme views of certain Mendelian writers who have held so strongly to a principle of the conservation or fixity of factors that they do not allow of their modification even through crossing. It may be doubted whether this group of Mendelians is really a large one, but so far as they do exist the criticisms of Gates are likely to have the sympathy of his readers. It is, however, one thing to recognize the complexity and possible instability of protoplasm as to its stereochemistry and quite another to hold that stereochemical changes within a pure species can produce such great modifications of morphological structure as the mutationists would have us believe. And the œnotheras are so strongly under suspicion of genetic complexity through hybridism that we have a right to expect that evidence for mutation from this group will be most critically sifted and only employed where it is found in material of proven purity.

One may be Mendelian, firmly believing in the principle of segregation following an $F_1$ generation which is the principal tenet of Mendelism, and still admit the probability of modifications from time to time of the stereochemistry of germ plasm even in so-called "pure lines." That such changes may result in spontaneous modifications of structure seems reasonable on philosophical grounds and such modifications would constitute mutations since they are discontinuous. But it remains to be proven that such modifications affect changes in morphology to the degree claimed by the mutationists, although it may well be possible that numerous small mutations would in time have a cumulative effect readily recognized. Thus advances in evolution may come about through numerous small steps, as held by Darwin, and some of these may be mutations, but it seems probable, as so strongly argued by Weismann, Lotsy and others, that the chief causes of variation in higher animals and plants and the most important directions of evolution are determined through the mixing of diverse germ plasms with their complex interactions. Modifications of germ plasm through crossing, and mutations due to external chemical and physical factors, would be expected at times to work simultaneously, and in such cases it may become a difficult matter to distinguish their separate effects. Mutations even though small in degree would, however, if sufficiently numerous, work in time profound modifications of structure, and on this common ground the mutationists and the followers of Darwin seem to have the strongest hopes of reaching an agreement.

The final chapters, "A General Theory of Mutations" and "The Evolutionary Significance of Mutations," continue and elaborate the discussion which runs through the previous pages and thus largely summarize or expand the author's views. A bibliography of about 500 titles, of which 42 are by Gates, completes the work.

BRADLEY M. DAVIS

*A Text-book of Zoology for Universities, Colleges and Normal Schools.* By THOMAS WALTON GALLOWAY, Ph.D., Litt.D., Professor of Biology in the James Millikin University. Third edition, revised. P. Blakiston's Son & Company, Philadelphia.

It fell to the lot of the present writer to undertake a review of the first edition of this book[1] which was published in 1906. In paper, typography, binding, etc., the book is well made and attractive to look upon, a very deserved compliment to the publishers, who seldom allow anything inferior in the way of bookmaking to emanate from the house.

Intrinsically there is little change over the first edition, aside from the addition of some four chapters comprising about sixty-five pages more than the former book. The same plan

[1] SCIENCE, Vol. XXIV., p. 719.

of treatment which characterized the former has been continued. A slight change in the title, the former having been "A Text-book for Secondary Schools, Normal Schools and Colleges," may possibly reflect somewhat of the well-meant criticism of the reviewer, possibly some slight change of attitude on the part of the author, touching the attempt to comprise so large a scope of utility for a single book. The principal feature of the new edition calling for mention is that comprising the added chapters already referred to. These the author designates a "third part, a group of synthetic chapters (XXVI.–XXIX.) to induce the student to gather up the details of his course by a new reorganization of the materials." The captions of the chapters will indicate their scope. Chapter XXVI., "The Doctrines of Evolution and Related Ideas," among which are heredity and Mendelism. Chapter XXVII., a very interesting one, is devoted to "Economic Zoology," including such features as "Animals as a Food Supply, Animals as a Source of Clothing for Man, Animals in Science and Medicine," etc. Chapter XXVIII., "Development of Zoology," is a brief summary of the history of zoology, including the "Greek and Roman Periods, the Middle Ages, Modern Period and Its Specializations," and ending with sections of the "Philosophy of Biology, and Applications of Biology."

It is a matter of regret that some of the errors pointed out in the previous edition have been allowed to go uncorrected. For example, the obvious error in the description of expanded and contracted conditions of *Vorticella* in Fig. 70 (68 of the first edition). Perhaps the error was so obvious as to be regarded beyond the necessity of correction, assuming that every one concerned would make it for himself! Again, the pleasing bit of biological fiction involved in the symbiotic relations of certain hydroids and the hermit crab, that "the polyps cover up the shell occupied by the crab, thus concealing it from its enemies and its prey," the unwarranted assumption of which was shown in the former review, remains in spite of reviewer, or well-known facts to the contrary. In this connection may also be pointed out that the illustrations of hydroid ontogeny shown in Fig. 84 are likewise of extremely doubtful validity, as are also other features in connection with the treatment of the cœlenterates.

In conclusion the reviewer would incline to question the assumption of the author (Preface, p. v) "that the right text-book of zoology, as of every other subject, is primarily a matter of psychology." It would be futile to discuss this proposition in this connection, but it seems fairly evident that there may be intrinsic and inherent principles which determine, quite as much as any psychological quirks, the method and content of a zoological text-book.

C. W. H.

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## *PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES*

The tenth number of Volume 1 of the *Proceedings of the National Academy of Sciences* contains the following articles:

1. *The Octopus Motive in Ancient Chiriquian Art:* George Grant MacCurdy, Peabody Museum, Yale University.

After discussing general features of animal motives in Chiriquian art, the octopus motive, which appears hitherto not to have been identified, is traced through a number of varying forms in vases, of which six are figured in cuts.

2. *The Life Cycle of Trypanosoma brucei in the Rat and in Rat Plasma:* Rh. Erdmann, Osborn Zoological Laboratory, Yale University.

The method employed affords the means of following, outside the body of the host, the sequence of changes in the life of trypanosomes, and its use has shown dimorphic forms, latent or round, and crithidia-like forms in *Trypanosoma brucei* outside of the host.

3. *The Effect of Pressure on Polymorphic Transitions:* P. W. Bridgman, Jefferson Physical Laboratory, Harvard University.

This note presents, in a compact form by means of diagrams, many of the essential facts concerning the effect of high hydrostatic pres-

sure on the polymorphic transitions of 30 substances.

4. *On Isothermally Conjugate Nets of Space Curves:* GABRIEL M. GREEN, Department of Mathematics, Harvard University.

A necessary and sufficient condition that a conjugate net of curves on a surface be isothermally conjugate is that at each point of the surface the pair of axis tangents, the pair of associate conjugate tangents, and the pair of anti-ray tangents be pairs of the same involution.

5. *The Rôle of the Liver in Acute Polycythaemia: The Mechanism Controlling the Red Corpuscle Content of the Blood:* PAUL D. LAMSON, Pharmacological Laboratory, Johns Hopkins University.

There is in the body a mechanism for regulating the red corpuscle content of the blood; this mechanism is under nervous control, responding to nervous, chemical and emotional stimuli; the adrenal glands play a part in this mechanism, and the liver is the seat of the changes which increase the number of red cells, partly by a reduction in plasma volume, and partly by bringing cells into the circulation which are not normally present.

6. *The Potentials at the Junctions of Salt Solutions:* DUNCAN A. MACINNES, Laboratory of Physical Chemistry, University of Illinois.

The author calls attention to the fact that the liquid junction potential $E_L$ of a concentration-cell of the type $Ag + AgCl$, $KCl(C_1)$, $KCl\ (C_2)$, $AgCl + Ag$ can be derived from measurements of its electromotive force $E$ and of the cation-transference number $n_c$ with the aid of the equation $E_L/E = (2n_c - 1)/2n_c$. This equation involves only the assumption that the work attending the transfer from one concentration to the other of one equivalent of ion is the same for the cation as for the anion. The author substantiates this assumption by showing that this equation, when applied to the electromotive force data of Jahn, leads to nearly the same values of $E - E_L$ (which should equal the difference in the two electrode-potentials) whether the electrolyte be KCl, NaCl or HCl.

7. *A Statistical Study of the Visual Double Stars in the Northern Sky:* ROBERT G. AITKEN, Lick Observatory, University of California.

At least one in every 18, on the average, of the stars as bright as 9.0 magnitude in the northern half of the sky is a double star visible with the 36-inch telescope. Close visual double stars are relatively more numerous in the Milky Way than elsewhere in the sky, and visual double stars as a rule revolve in relatively small orbits. Close visual double stars are rare among stars of either very early or very late spectral class.

8. *Walnut Mutant Investigations:* ERNEST B. BABCOCK, Division of Genetics, Department of Agriculture, University of California.

The mutation takes place in female flowers only and appears in the first generation after the mutation occurs but on crossing with the species type it is completely recessive in the $F_1$ generation and the nature of the mutation is such that only certain genetic factors are affected without having the chromosome number disturbed.

9. *Hereditary Fragility of Bone:* C. B. DAVENPORT AND H. S. CONARD, Carnegie Institution of Washington and Grinnell College, Iowa.

Of a parent who early in life was affected with brittle bones at least half the children will be similarly affected, but if neither parent, though of affected stock, has shown the tendency, then expectation is that none of the children will have brittle bones.

E. B. WILSON

## SPECIAL ARTICLES

### ON THE OCCURRENCE IN THE SOUTHERN HEMISPHERE OF THE BASKING OR BONE SHARK, CETORHINUS MAXIMUS

SINCE it does not seem generally to be known that this giant elasmobranch is found in southern waters, it may be of interest briefly to give the following data which have been noted from time to time in the course of other ichthyological studies.

The old writers thought that this great beast was confined to the far north Atlantic and to the Arctic Ocean. Of these, Friedrich Faber

may be quoted. In his Natural History of the Fishes of Iceland he says:

> This giant among sharks [and he quotes Gunner that it grows to a length of 16 fathoms (Klaftern), but adds for himself that its ordinary length runs from 20–24 feet] is found only in northern seas and, moreover, is here not abundant.[1]

Goode and Bean in their monograph, "Ocean Ichthyology"[2] (1895) write:

> It is unknown elsewhere than in the north Atlantic, and south of the Grand Banks on the west, and Scotland on the east may be regarded as an estray.''

Jordan and Evermann (1896)[3] give it a more extended habitat since it is "found in Arctic waters, straying south to Portugal, Virginia and California." And later Jordan (1905)[4] in his "Guide to the Study of Fishes" notes that it occurs "on all northern coasts (and is) most frequently taken in the North Sea and about Monterey Bay, California." However, Bridge (1904)[5] in the Cambridge Natural History writes:

> Although generally described as a northern form, *Cetorhinus* is known to occur in Australian waters.

As might be expected from the well-known activity in ichthyological matters of the Australians, our southern references are mainly confined to the waters surrounding that continent.

Its occurrence in the antipodes was, so far as the present writer knows, first made known by Sir Frederick McCoy[6] in 1885. He figures and describes a large specimen taken at Portland on the west coast of Victoria in November, 1883. It was 30 feet 6 inches long and 20 feet in girth. He gives a very careful and minute description and very detailed measurements, the first and with one exception the only ones known to the present writer.

William Macleay,[7] whose paper was apparently published in the same year as McCoy's but slightly later, merely lists the basking shark in Australian waters on the authority of the latter, and gives no new data.

We next hear of *Cetorhinus* in 1901, when two specimens were taken at Eden, New South Wales, and were recorded by Mr. E. R. Waite in the following year.[8] Of the first specimen only a piece of "baleen" or gill-rakers reached Mr. Waite, but this was sufficient for its identification. However, in August, 1901, a young male 10½ feet long was taken at the same place and sent to the Australian Museum, Sydney.

A more extended account of the occurrence of this great shark in these southern waters is from the pen of Mr. J. A. Kershaw[9] of the National Museum at Melbourne, Australia. His specimen, which was a young male 12 feet and 11 inches long, was taken in Hobson's Bay near Williamstown, Victoria, in May, 1903. Mr. Kershaw gives a series of very full and carefully made measurements, the only one except McCoy's that seem ever to have been recorded. Its length lacked but one inch of 13 feet and its girth in front of the first dorsal was 3 feet 7 inches. Of Kershaw's careful description the most noteworthy point refers to the snout, which will be discussed later.

Stevenson (1904)[10] says that the basking

[1] Faber, Friedrich, "Naturgeschichte der Fische Islands," p. 20. Frankfurt am Main, 1829.

[2] Goode, George Brown, and Tarleton H. Bean, "Ocean Ichthyology," p. 22. Washington, 1895.

[3] Jordan, David Starr, and Barton W. Evermann, "The Fishes of North and Middle America," Vol. I., p. 51. Washington, 1896.

[4] Jordan, David Starr, "A Guide to the Study of Fishes," Vol. I., p. 540. New York, 1905.

[5] Bridge, T. W., "Fishes" in the Cambridge Natural History, Vol. VII., p. 453. London, 1904.

[6] McCoy, Frederick, "Natural History of Victoria. Prodromus of the Zoology of Victoria," Decade XI., pp. 11–15, Plate 104, 1885.

[7] Macleay, William, "Supplement to Descriptive Catalogue of the Fishes of Australia," *Proceedings Linnæan Society of New South Wales*, Vol. IX., pp. 62–63.

[8] Waite, E. R., "New Records or Recurrences of Rare Fishes from Eastern Australia," Records Australian Museum, Vol. IV., p. 263, 1902.

[9] Kershaw, James A., "Notes on a Rare Victorian Shark," *The Victorian Naturalist*, Vol. XIX., p. 62, 1903.

[10] Stevenson, Charles H., "Aquatic Products in Arts and Industries," Report U. S. Fish Commission, Vol. XXVIII. for 1902, pp. 227–228, 1904.

shark is found on the coasts of Australia, Ecuador and Peru. So abundant are they in the latter region that a profitable fishery is carried on, their livers being utilized for oil. Stevenson quotes a Captain Baker, of New Bedford, that he took 125 barrels of basking-shark liver oil in 2 days. Stevenson further says that this great shark is reported to be taken at Kurrachee in northwest British India. All the authorities are agreed, however, that this latter shark is *Rhineodon typus*, the whale shark.

Last of all, Stead (1906)[11] notes that *Cetorhinus* is found on the coasts of New South Wales and Victoria, but not abundantly. This statement is probably based on the accounts of McCoy, Waite and Kershaw.

Even McCoy's notice is, however, *not* the first on the occurrence of this great shark in the South Seas, for Bennett (1840)[12] in his zoology of the voyage of the whaleship *Tuscan* tells us that:

> While cruising in the south Pacific, we occasionally observed large animals which bore a great resemblance to whales (excepting that their tail-fin was perpendicular, and they did not spout), swimming near the surface of the sea. They appeared to be nearly 20 feet in length, and were called by the whalers bone-sharks, a name which implies little more than the very vague idea entertained of their true character. They are said to have whalebone in the mouth, yet do not spout; but partake of the nature of a shark, or other fish, and, like fish, can maintain a submarine existence for an indefinite time. They have been occasionally mistaken for whales, and harpooned by inexperienced whalers, when, taking away the line with irresistible impetuosity, they have disappeared in the ocean's depths, and left their assailants to watch in vain for their return to the surface.
>
> Since whalers religiously avoid an encounter with these troublesome creatures, it follows that their real form and structure are but little known. If we admit that an error exists on the subject of there being whalebone in its mouth, it appears probable that the bone-shark is allied to, or is identical with, the Basking-shark (*Squalus maximus*), a fish, measuring from 15 to 30 feet in length, and which was formerly regarded as a species of whale.

Before bringing this note to a close, I wish to call attention to one other matter of importance. None of the observers save Kershaw correctly gives the shape of the snout. Günther (1880) indeed says

> . . . young specimens have a much longer and more pointed snout than adults. . . .

And McCoy notes that it has a

> fusiform head very small, abruptly narrowed to a short snout, with a slightly concave profile rising from a little behind the eye . . .,

all of which structures are shown in his figure, the snout appearing as a very weak and ineffective structure.

Goode and Bean (1895) merely say "snout blunt," and their figure, which is copied from Annales du Musée d'Hist. Nat. Paris, Vol. XVIII., pl. 6; and from Fish. Ind., pl. 249, upper figure, and which in turn has been widely copied (and is the only one save McCoy's known to the present writer), so shows it. Jordan and Evermann (1896), for example, simply copy Goode and Bean's text and figure. So practically does Bridge (1904). While Jordan (1905) calls it elephant shark but assigns no reason for the name.

Kershaw (1903) specifically notes that

> . . . the front of the head is considerably extended, and forms a thick, fleshy, truncated snout, with the extremity produced into a curved fleshy hook, which altogether gives the fish a most extraordinary appearance.
>
> This peculiarity, according to some earlier observers, occurs only in the young specimens, and has led to the erroneous opinion[13] that several different genera and species of basking sharks existed, an opinion which can hardly be wondered at considering the greatly different appearance this gives to the fish.

This specimen was a young one, only 12 feet 11 inches long; however, a year previous a fisherman had reported to Kershaw the capture in Melbourne Harbor of a large shark having "a long thick snout terminating in a hook."

[11] Stead, David G., "Fishes of Australia," pp. 233, 235, 236, 251. Sydney, 1906.

[12] Bennett, F. D., "Narrative of a Whaling Voyage Round the Globe, from the Year 1833 to 1836," Vol. II., London, 1840.

[13] Günther, A. C. L., "Introduction to the Study of Fishes," p. 323, Edinburgh, 1880.

The present writer has recently had an opportunity to examine in a traveling museum a mounted 36-foot specimen of *Cetorhinus maximus* taken in Monterey Bay, California. This had a thick and stout but very marked snout, bluntly conical in shape, which projected at least 15 inches in front of the upper jaw and from 18–20 inches over the lower jaw. It is interesting that such a marked structure should have so long escaped notice, but on the other hand opportunities to examine this giant shark come very rarely to properly trained naturalists. However, it seems from the above data that Jordan's name, elephant shark, is by no means a misnomer.

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## Labeling Chemical Specimens

Probably every teacher of chemistry makes some use of samples of chemical elements and compounds in his lectures. In some cases, the set of specimens may have been purchased complete, in uniform style containers, with systematic and uniform labeling. Quite often, however, the additions to a chemical museum are made gradually, and as a result the collection may consist of all sorts, sizes and varieties of containers with an equally varied assortment of labels. When a set of specimens grows in this way, one can scarcely make use of serial numbers alone. An expansible system is necessary. The writer has used such a system for several years, with increasing satisfaction.

This labeling system has so far been used only for elements and inorganic compounds. It corresponds in principle to the usual library method of labeling books. A chemical catalogue of a leading firm was used as the source of the names. The list of chemicals being really quite comprehensive, it was possible to give a label number to the name of each substance one would ever be likely to include in a collection, writing these label numbers directly into the catalogue. The list of the chemical elements, in alphabetical order, was taken as the starting point. Names of elements beginning with the same letter are given serial numbers, as for example, magnesium is called M 1; manganese, M 2; mercury, M 3; molybdenum, M 4; aluminium, A 1; and ammonium compounds are placed under A 2. Specimens of the elements are labeled thus: Aluminium, A 1.0. When several kinds of samples of an element are included they are labeled thus: Aluminium, A 1.0 sh; aluminium, A 1.0 po; aluminium, A 1.0 le; the letters "sh," "po" and "le" stand respectively for "sheet," "powder" and "leaf." The method of labeling compounds may be illustrated by a few from the sodium list:

| | |
|---|---|
| Sodium acetate, cryst. | S 6 a |
| Sodium acetate, fused | S 6 a 2 |
| Sodium acid carbonate | S 6 ca |
| Sodium carbonate, dry | S 6 ca 4 |
| Sodium chlorate | S 6 ch |
| Sodium chloride | S 6 chl |
| Sodium cyanide | S 6 cy |
| Sodium iodate | S 6 io |
| Sodium iodide | S 6 io 2 |
| Sodium acid sulphate, powd. | S 6 su |
| Sodium acid sulphate, fused | S 6 su 3 |
| Sodium sulphate, cryst. | S 6 su 4 |
| Sodium sulphate, powd. | S 6 su 5 |
| Sodium sulphide | S 6 sul |
| Sodium acid sulphite | S 6 sul 3 |
| Sodium sulphite, cryst. | S 6 sul 4 |
| Sodium sulphite, powd. | S 6 sul 5 |
| Sodium sulphocyanate | S 6 sul 6 |
| Sodium thiosulphate, cryst. | S 6 th |
| Sodium thiosulphate, powd. | S 6 th 2 |

Where omissions occur, as between "S 6 ca" and "S 6 ca 4," it is understood that other compounds or different forms of the same compound are to be supplied. These are, of course, to be found in the complete chemical catalogue.

From these examples it may be seen that the bottles, marked in this way, can always be kept in alphabetical order, and can scarcely be misplaced if the labels are read as to letters and numbers. Any sort of helper, who knows his alphabet and can count, will be able to take out specimens and replace them without confusion. New samples can be easily inserted anywhere, and given a label which shows exactly where it belongs.

No originality is, of course, claimed for this system of labeling, but the writer does not happen to know of its being used elsewhere for chemical specimens. Any one may easily devise letters and numbers to fit his present collection as well as future additions. Provision may be made for alloys, commercial samples, and the like, wherever necessary.

It is convenient to write the letters and figures in two lines, library style, as, "S 6" is written large with the "th 2" written small, beneath. Three figures, with the initial, will be the maximum number of characters in the first line. For the lower line, the rule is not to use more than three letters, while two figures will always be sufficient.

The bottles used in the writer's collection are one-pound and half-pound "salt mouth," of uniform style, with "mushroom" stoppers. These are convenient sizes, the smaller size being used mainly for the more costly substances. There are two labels on each bottle. The larger labels are known to the trade as No. 1006, about four by one and five eighths inches. The names of the substances are printed on these labels with rubber type in capital letters a quarter of an inch high. These labels are placed just below the shoulder of the bottle. The round labels used for the index letters and numbers are known as "A 88" or library labels. These are centered under the large label near the bottom of the bottle. A library assistant did the lettering of these labels, using india ink. The bottles are sealed with paraffin, and the labels coated with paraffin. The latter is necessary as the bottles are kept on open shelves, and usually require wiping with a damp cloth when they are to be shown. The paraffin further protects the labels against accidental contact with acids or alkalies.

This system of labeling is scarcely applicable to organic compounds, unless one does not wish to keep them separate from inorganic. The writer, for present purposes, has made a list of the substances studied or referred to, in order, in the organic text used (one of the most complete published), and each substance given a number. This does not include substances that are impracticable to keep or procure. The collection is so arranged that the substances mentioned in a given chapter are found together, in numerical order, the missing numbers to be supplied in the future. Another possible arrangement is by classes of compounds. This has been used, but is rather less convenient than the present arrangement. In the organic set, the half-pound bottle is the maximum size for solids, the two-ounce the minimum. For liquids, there are three sizes, from a half-liter down. The labels are No. 1007 for the names, and No. 539 for the numbers. The bottles are paraffined as in the inorganic set. On account of the effect of light on many organic compounds, the specimens are kept in a dark room, where the inorganic set is also kept for convenience.

The "looks" of one's teaching devices will be certain to leave lasting impressions on the observer. This is especially true in the chemical laboratory and lecture room. While it is not claimed that the above-described methods of labeling bottled chemical specimens are the best that could be devised, they have served the writer very well and it is hoped that the description may interest others.

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COLORADO AGRICULTURAL COLLEGE

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## *REPORT OF THE SAN FRANCISCO MEETING OF SECTION F OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. II*

*Thursday, August 5*

*Morning Session, Demonstrations*

In charge of OLIVE SWEZY, University of California

*Entamœba Buccalis,* Inez F. Smith, University of California.

Mitosis and Multiple Fission in the Flagellata, Olive Swezy, University of California.

Mitosis in *Lamblia muris,* Elizabeth Christiansen, University of California.

Enflagellating and Exflagellating Soil Amœba, Charlie W. Wilson, University of California.

Flagellates of Hemiptera, Irene McCulloch, University of California.

Drawings for Monograph on Dinoflagellata, C. A. Kofoid and Mrs. Rigden-Michener, University of California.

Drawings for Monograph on Pacific Tintinnoidea, C. A. Kofoid and Mrs. Elizabeth Purington, University of California.

Balantidium from the Pig, J. D. MacDonald, University of California.

Drawings of Elasmobranchs, J. Frank Daniels, University of California.

*Papers: Protozoology*

CHARLES A. KOFOID, University of California, presiding

*Chromosomes in Protozoa:* MAYNARD M. METCALF, Oberlin, Ohio.

This review has endeavored to show that in each group of the Protozoa are found elongated chromosomes which are linear aggregates of granules and which split longitudinally in mitosis, giving exact equivalence of the daughter nuclei. In all major groups, except perhaps the *Mastigophora*, presexual and sexual phenomena, essentially similar to those of *Metazoa* are known. The Metazoan mechanism of inheritance is therefore present, in some representatives at least, of all the great Protozoan groups. It has shown also that, in some representatives, at least, of the *Plasmodrome*, during the vegetative phases of the life history, the chromatin outside the caryosome, indeed apparently outside the caryoli, is thrown off as trophical chromidia. Such evolution of complexity of organization as has occurred in the *Metazoa* probably could not occur until, as in the *Ciliata*, a considerable amount of chromatin is kept intact throughout the whole life history, including its vegetative as well as its presexual and sexual phases. The lower Protozoa have the Metazoan type of mechanism of inheritance in connection with their sexual phases but can not utilize it for the development of a complex series of determiners for elaborate structural organization, for they do not keep this mechanism intact during their vegetative periods. The higher Protozoa apparently have the Metazoan type of mechanism of inheritance and keep it intact through all their life cycle. To what extent they have utilized its presence to develop determiners, is an interesting question which does not seem beyond experimental study with favorable material.

*Problems on Rejuvenescence in Protozoa* (illustrated with lantern slides): LORANDE L. WOODRUFF, Yale University (read by title).

*The Evolution of the Protozoan Nucleus and Its Extranuclear Connections:* CHARLES ATWOOD KOFOID, University of California.

The essential and fundamental similarity of Protozoan and metazoan nucleus is indicated by the general trend of recent cytological investigations among the Protozoa. The process of mitosis has the same sequence of phases, though the chronology of mitotic events and of the division of extranuclear structures varies from species to species and also among individuals within the species. In the early prophase in trichomonad flagellates and in *Nægleria* the chromatin aggregates, presumably chromosomes each split longitudinally, or the chromatin network forms a split skein or thread which later fuses into one, or emerges on the equatorial plate in chromosomes in which the precocious splitting has entirely disappeared. The chromosomes are definite in number and are differentiated among themselves as to size and behavior in mitosis. There are suggestions of odd and even numbers (4, 5) there is (in Trichomonad flagellates in several genera and species) a small chromosome lagging in the metaphase, and there are instances of unequal division of chromosomes.

The blepharoplast contains the centrosome in trichomonad flagellates or is attached to it (*Giardia-Lamblia*). This organelle should not be called a kinetonucleus; Schaudinn's report of its heteropole origin by mitosis is unsupported and probably invalid. It arises in enflagellating *Nægleria* (*-Amœba*) *gruberi* from the centrosome or centriole in the central caryosome, which sends out a radial fibril which enlarges at the periphery into a blepharoplast at the base of the two flagellæ which grow out from this enlargement. In exflagellation the flagella withdraw and together with the blepharoplast retreat into the caryosome. The blepharoplast is highly developed in parasitic flagellates and is directly connected by a system of fibrils with nucleus, flagella, axostyle and parabasal body, the whole forming an integrated structural unit which functions as a neuromotor apparatus comparable with that of the ciliate *Diplodinium*. It is a specialized structure developed to the greatest extent in connection with parasitic life demanding unusual expenditure of energy in locomotor activity. The term kinetonucleus should be abandoned as misleading. The Binuclearity Hypothesis in its nuclear implications has no adequate foundations, and the order Binucleata should be discarded. The parabasal body of flagellates and *Trichonyphida* is perhaps the analogue of the macronucleus of the ciliates and is an extranuclear store of the chromatin related to the needs of flagellar and ciliary activities.

*Afternoon Session Papers: Geographic Distribution*

JOSEPH GRINNELL, University of California, presiding

*California as a Testing Ground for Theories of Distributional Control:* JOSEPH GRINNELL, University of California.

*Insect Transmission of Swamp Fever:* J. W. SCOTT, University of Wyoming.

During the summer of 1914, the writer obtained experimentally a well-defined case of swamp fever, and the conditions of the experiment leave no doubt that the disease was contracted through the agency of certain biting insects. This disease, frequently known as "infectious" or pernicious anemia, is a serious and destructive blood disease of the horse. It has been reported from France, Germany and Japan, and is widely distributed in North America, from Texas to the Northwest Territories, and from the states of Washington and Nevada to the Province of Ontario, Canada. It has an altitudinal distribution from near sea-level to at least 9,000 feet. It is usually found in swampy regions, but has also been reported from rolling, wooded countries. Wherever found, it may become epidemic and cause the loss of a large percentage of a given herd of horses. The disease shows a seasonal distribution, reaching a maximum number of cases in late summer or early autumn. The disease itself is characterized by progressive emaciation and an intermittent rise in temperature; it is frequently accompanied by anemia, and the rises in temperature are sometimes at quite regular intervals. The organism is filterable, for the disease is transmitted by the injection of blood serum after it has been passed through a Berkfeld filter.

In France, Vallei and Carri concluded that natural transmisson took place through drinking water contaminated with urine or feces from an infected horse. Van Es, of North Dakota, in 1911, after a study of several years, thought this the most probable explanation. Swingle, however, in Wyoming, in 1912 and 1913, showed that it was an extremely difficult matter to secure infection by way of the alimentary canal, his numerous experiments with urine and feces all resulting negatively.

At this point in 1913 the writer began the problem. Since internal transmission was a difficult matter, only one or two cases being known (Van Es, Schlatholter), and since the contamination of drinking water could hardly explain epidemics, it was believed that natural transmission must be by means of some external agent. Accordingly in the spring of 1914 a screened cage was erected capable of holding five horses. The cage had an entryway 10 feet long, each end closed by a door and the screen had 16 meshes to the inch.

The first experiment in this cage was with various kinds of mosquitoes and resulted negatively. A longer and more conclusive experiment with mosquitoes the present summer has had a similar result. There was next introduced into this cage a considerable number of flies; these were house-flies, stable-flies and a few other wild flies, including one of the smaller species of Tabanids. The house-flies and stable flies thrived and increased rapidly in numbers between the first and the twenty-fifth of August; the other flies soon died and were not observed to attack the horses. The stable-flies were observed to feed in large numbers on both infected and well horses. Except for two or three days infected horses were kept continuously in the cage from July 27 to August 28. Three well horses were exposed in the cage during this time. On August 28 horse No. 22, a healthy strong animal, showed a temperature of 102.8. After two more fever periods this horse died October 5. Subinjection of his blood has produced typical cases, showing that he had the disease. The temperature of this horse had been normal from June 9 to August 28, and he had not been outside of the cage for 25 days, while 10–14 days is the ordinary incubation period of the disease. Under these conditions there appears to be no escape from the conclusion that insects transmitted the disease. It is believed further that the stable-fly was responsible for the transmission for the following reasons: (1) These flies were observed to attack the horses viciously. (2) Negative results of two other experiments show that the mosquitoes were not responsible, even though a few were still in the cage during August. (3) Houseflies do not bite, and the other flies present in the cage did not attack the horses, so far as one could observe, and soon died.

The experiment, with the stable flies alone, is being repeated, and another experiment involving some of the Tabanidæ is also in progress.

*The Big Bears of Western North America, with Special Reference to their Distribution:* C. HART MERRIAM, United States Biological Survey.

The bears are the largest of living carnivores and are widely distributed, being found in both Americas and in Eurasia. The typical genus *Ursus* occurs in both eastern and western hemispheres. South America is the home of *Tremarctos,*

a short-faced bear resembling *Arctotherium*, of the American Pleistocene, while the north polar regions are inhabited by the polar bear, *Thalarctos*. The ancestry of the bears is still clouded, none being known below the Pliocene.

Systematically the bears are a compact group, yet within the genus *Ursus* several subgroups may be recognized; the subgenus *Euarctos*, containing the black or cinnamon bears and the subgenus *Ursus*, containing the grizzly and brown bears. Of the latter (grizzly and brown bears) two, three and even four species, representing different species-assemblages, may be found in a single locality, as in the fossil deposits of Rancho La Brea or in Yellowstone National Park. The characters used in identification are chiefly cranial and dental. Skulls of males of the grizzly group are two, and in some cases, three times the bulk of those of females of the same species. At present about 40 species of grizzlies and 10 of brown bears are recognized, where formerly but one of each was known. For example, California once contained representatives of five different groups of grizzlies. The recognition of this large number of species has been made possible by the extensive collections of skulls in the United States National Museum and in the California Museum of Vertebrate Zoology.

*Fossil Tertiary Mollusca of the Rocky Mountain Region:* T. D. A. Cockerell, University of Colorado, Boulder, Colorado.

The recent expeditions of the American Museum of Natural History, primarily for the discovery of mammalian remains, have brought to light some very interesting Tertiary land and freshwater shells, principally in Wyoming and New Mexico. From a study of these we are led to the following conclusions:

1. Certain of the most characteristic genera of the Rocky Mountain land shells living to-day are apparently very ancient inhabitants of the same general region, and have, perhaps, at no time extended very much beyond it. The most noteworthy example is *Oreohelix*, represented in the Eocene and Paleocene by large species, some of the specimens showing the sculpture of the apical whorls, which agrees with that of the group called *Radiocentrum* by Pilsbry. A species of *Holospira* from the New Mexico Paleocene is extremely like a living species of Arizona.

2. *Ashmunella*, one of the most characteristic endemic genera of the southwest, is not at present known below the Pleistocene. It is not possible to be sure, at present, whether all the peculiar genera of the Rocky Mountain region and southwest are very ancient inhabitants of that country, but it seems very likely that they will all be found, sooner or later, in the early tertiaries.

3. Various circumpolar genera of small snails, such as *Pupilla* and *Cochlicopa*, represented in the modern fauna by species identical or nearly identical with those of Europe and northern Asia, are apparently lacking in the Tertiary, or at any rate in the Eocene. They may have been overlooked, but it is probable that they have reached our region in much more recent times, from Eurasia, as their small amount of modification, or total lack of it, would suggest the Eocene collections do contain small species of more characteristically American types as *Vitrea* and *Thysanophora*.

4. The Paleocene and Eocene faunas included a series of genera entirely different from anything now living in the same region, but evidently related, at least in part, to the present Central American and West Indian faunas. Thus we have from the Eocene of Wyoming species apparently referable to *Pleurodonte* and *Eucalodium*. It is not clear, at the present time, whether this Central American fauna originated northward, or whether it had its main center in the region where it now exists, merely extending northward during a time when the country now represented by Wyoming and Colorado was low, moist and warm.

5. The most remarkable discoveries have been of small shells belonging to the Bulimulidæ, having the aperture or last whorl upturned, representing at least two genera (*Protoboysia* and *Grangerella*) and four species. These, while belonging to extinct genera, show evident relationship with some of the South American snails, and at the same time a remarkable resemblance to the Indian genus *Boysia*, which is supposed to belong to the Pupillida, though the anatomy is unknown.

6. Among the fresh-water shells, the Unionidæ are the most interesting. About the end of the Cretaceous and beginning of the Tertiary we find in the Rocky Mountain region a large and varied series, resembling the types which now inhabit the Mississippi Valley. At about the same time that the dinosaurs disappeared, these mussels also departed, leaving an impoverished Unionid fauna in the Eocene, which in its turn eventually died out altogether. It seems probable that this change was connected with earth movements.

*Isolation as a Factor in the Evolution of Thais lamellosa* (illustrated with specimens and lantern slides): Trevor Kincaid, University of Washington.

*Friday, August 6*

*Morning Session, Demonstrations*

In charge of F. W. WEYMOUTH

Living Ova of Rat in Serum, J. A. Long, University of California.

*Papers: Marine Zoology*

F. M. MCFARLAND, Stanford University, presiding

*The Occurrence and Possible Causes of Periodic Vertical Movements of Aquatic Organisms:* C. O. ESTERLY, Occidental College, Los Angeles, California.

It is well known that many aquatic organisms, particularly those of the plankton, are more abundant at or near the surface by day than by night. This is because they ascend from deeper water in the early part of the night and descend from the surface later.

In order to understand the fundamental causes of this phenomenon it is necessary to have accurate knowledge of the field conditions under which collections are made, particularly of the temperature and salinity of the water and of the light intensity as represented by the time of day at least. It is highly important that field observations be supplemented by laboratory experiment to determine to what sorts and degrees of stimuli the organisms respond. Furthermore, each species must be studied by itself.

The explanation of the diurnal movement that at present is most satisfactory is the one based on responses to different factors in the environment, but our knowledge in this respect is very incomplete. The "mechanical explanation" which takes into account mainly the changes in viscosity of the water does not satisfy. The suggestion that the alternating rise and fall is due to metabolic rhythms has received so little attention that its worth is not apparent.

*Some Physiological Characters of Marine Animals from Different Depths:* V. E. SHELFORD, University of Illinois, Urbana.

*Field Study of Animal Behavior as contrasted with Laboratory Study:* ELLIS L. MICHAEL, Scripps Institute, La Jolla, California.

The object of this paper is to emphasize the necessity of employing two essentially distinct but mutually helpful methods of research in any strictly scientific study of animal behavior. All biologists readily admit that an important function of the well-known laboratory method is that of analyzing the mechanism involved in an organism's response. Few, however, seem to realize that this method is incapable of revealing how any particular species is related to its environmental complex.

Yet, the minute a definite answer to such a question is sought, a little thought clearly shows that recourse must be had to some method of field observation. For instance, could any amount of laboratory experimenting reveal the fact that *Sagitta bipunctata* is more abundant between 15 and 25 fathoms than at any other depth, or that it increases in abundance as the distance from the coast decreases?

In order to demonstrate the indispensability of a field method three aspects of the behavior of *S. bipunctata* are illustrated in some detail. First, the variation in abundance at all depths at different times of day is considered, showing how the species migrates vertically. Second, the fact that it accumulates in all depths in greatest numbers when the temperature lies between 13° and 16° C. is revealed. Third, data are presented showing that the species is more abundant, on the surface at least, when the salinity lies between 33.55 and 33.70.

Finally, the question of the interpretation of the results is discussed. Attention is called to the fact that an adequate interpretation necessitates recourse to the laboratory method. Obviously the field method can not, except by interference, ascertain the nature of response involved, *i. e.*, as to whether the demonstrated relations are due to a tropism, a taxis, a metabolic reaction, or to a direct physical effect of changes in viscosity and a specific gravity.

*The Influence of Chance on the Number of Organisms Collected in Plankton Nets:* GEORGE F. MCEWEN, Scripps Institute for Biological Research, La Jolla, California.

Measurements of a number of hydrographic elements and the corresponding abundance, or number of a species per unit volume of water in the ocean supply the data for determining the way in which the distribution of the species is related to these elements.

Practically, the distribution of the smaller organisms must be inferred from the number of each species collected per haul made with a plankton net. A plankton net filters rather variable fraction of the water that would pass through the net rim, if unobstructed; also the estimate of the distance hauled is usually subject to accidental errors of importance.

Grouping values of the abundance thus obtained with respect to each of a series of magnitudes of one of the environmental elements exhibits in tabular form the relation of the distribution to that element and the others linked with it. The errors in estimating the abundance arising from the use of a plankton net, the variability of the elements other than the one in question, and the fact that only a relatively small sample of the total population is examined have an important accidental influence on the character of the relation.

Illustrations are given of a method of testing the significance of the difference between two values of the abundance, which depends upon the probability that a difference in the same direction would arise by chance if the number of hauls were indefinitely increased. A plan is briefly outlined for eliminating the effects of all but one of the elements and testing the significance of the relation indicated by the corrected values of the abundance. The same methods are also applicable to statistical treatment of other kinds of quantitative data.

*The Boring Mollusca of the Pacific Coast:* Mrs. Ida S. Oldroyd, Long Beach, California.

*The Life History of the Pacific Herring:* C. McLean Fraser.

The Pacific herring appear in large schools all along the Pacific coast from California to Alaska. The average weight of those caught in purse seines is about 3.6 oz. and the length 8 inches, not including caudal finrays. They wander about in search of food, which consists mainly of copepods. There is but one spawning season in one locality. Near the biological station, Nanaimo, this is in February and March. Spawning takes place in shallow water, but this may be incidental to the requirement of barnacle larvæ for food at that time. Both females and males rub against the sea weed while spawning. The spawn adheres, hatches out in 14 or 16 days and the yolk lasts another six days. First spawning takes place at the age of three or four years. Most herring caught are from four to eight years; some were found ten years old. The scale increases in size in approximately the same ratio as the other parts of the body and the different year growths are marked off by winter checks or rings. Those who have calculated the growth of the fish in each year from the rate of growth of the scales have failed to take into account that the scale does not start to grow when the fish does. The herring is 3.5 cm. long before the appearance of the scale. This should be taken into consideration when the length of the fish is divided according to the divisions of the scale as shown by winter checks.

*The Nuclear Phenomena in Paramecium:* R. T. Young, University of North Dakota.

During the San Francisco meetings on Thursday, August 5, of the American Association for the Advancement of Science, there was formed a Pacific Branch of the American Society of Zoologists. The officers elected at this meeting were:

*President,* V. L. Kellogg, Stanford University, Palo Alto, California.

*Vice-president,* R. M. Yerkes, Santa Barbara, California.

*Secretary and Treasurer,* Joseph Grinnell, University of California, Berkeley, California.

*Executive Committee,* C. O. Esterly, Occidental College, Los Angeles, California; Barton W. Evermann, California Academy of Science, San Francisco, California; Charles L. Edwards, Los Angeles, California; J. Frank Daniel, University of California, Berkeley, California; Harold Heath, Stanford University, Palo Alto, California.

On Thursday, August 5, there was formed a Pacific Coast Branch of the American Society of Naturalists with the following organization:

*President,* Barton W. Evermann, California Academy of Sciences, San Francisco, California.

*Vice-president,* John F. Bovard, University of Oregon, Eugene, Oregon.

*Secretary,* Ellis Leroy Michael, Scripps Institute for Biological Research, La Jolla, California.

*Treasurer,* L. L. Burlingame, Stanford University, Palo Alto, California.

*Executive Committee,* Trevor Kincaid, University of Washington, Seattle, Wash.; Harry B. Torrey, Reed College, Portland, Oregon; Frank M. McFarland, Stanford University, Palo Alto, California.

The society will take the place of the local biological societies of the Pacific coast.

The Biological Society of the Pacific met at the Hotel Sutter, San Francisco, August 4, for their annual meeting. The address of the evening was given by Dr. Harry Beal Torrey, of Reed College, on "Research and the Elementary Student of Science." At this meeting the Biological Society voted to drop its organization in favor of the newly organized Pacific Branch of the American Society of Naturalists.

H. V. Neal,
*Secretary*